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ENDEAVOUR

The British quarterly scientific journal ENDEAVOUR was first published, by Imperial Chemical Industries Limited, in January 1942. Its purpose is to provide scientists, especially those overseas, with news of the progress of the sciences. While emphasis is laid upon British work, occasional articles from overseas contributors are included and impartial reference is made to the world's scientific literature. To make the journal truly international in character it is published in five separate editions—English, French, German, Italian, and Spanish.

No charge is made for ENDEAVOUR. It is distributed to senior scientists, scientific institutions, and libraries throughout the world, the guiding principle being that of helping scientists overseas to maintain those contacts which their British colleagues have always so much valued. Within these limits the Editors are at all times glad to consider the addition of new names to the mailing list.

The drawing on the cover is of the bark Endeavour, *which, commanded by Captain James Cook and carrying a number of scientific workers, was sent out by the British Admiralty in* 1768 *to chart the South Pacific Ocean and observe the transit of Venus*

ENDEAVOUR

A quarterly review, published in five languages,
designed to record the progress of the sciences
in the service of mankind

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Some distinguished Presidents of the Royal Society

The strength of a society lies in the collective ability of its members, but the effective deployment of its strength depends in large measure on its officers. Like the head of a state, the president should not only represent but personify his society. Over the three centuries of its existence, the Royal Society has been fortunate in having many Presidents who have both added lustre to its scientific reputation and helped to guide it through countless practical difficulties. Since its foundation, the Society has had some fifty Presidents, and the celebration of its Tercentenary is an appropriate time to recall their services to it.

In the Society's official record, Viscount Brouncker is named as the first President, holding office for five years from 15th July 1662, the date on which the first charter passed the Great Seal. But we are this year celebrating the Tercentenary of an earlier event, namely a meeting on 28th November 1660, at which the young Oxford Philosophical Society established a formal constitution. Foremost among those who later gained for this Society the royal patronage that was so important for its survival was Sir Robert Moray. In the early days the President was elected monthly, and until the granting of the first charter Moray held this office more frequently than any other, and has therefore a special claim to be remembered. Moray had been a loyal friend and servant of Charles I and in 1646 devised a plan for him to escape from Newcastle to the Continent, a plan that was frustrated only by a last-moment hesitation on the part of the monarch. Subsequently Moray himself spent some years in exile on the Continent and there occupied much of his time in scientific pursuits. After the restoration of the monarchy in May 1660, his political ability and his personal friendship with Charles II gave him much influence at court, and he did not spare himself in earning recognition for the Philosophical Society. While Moray was not a great scientist, there is no President to whom the Society owes a greater debt.

Brouncker was a well-known mathematician who also took a leading part in the activities that culminated in the Society's foundation. Another in the same group was Christopher Wren, President from 1680 to 1682. Wren was an astronomer —being professor of astronomy first at Gresham College, London, and then at Oxford—but is of course more generally known as a great architect.

For virtually the first quarter of the eighteenth century the President was Isaac Newton, elected in 1703 and remaining in office until his death in 1727. Of Newton no more need be said here than that he was not only the greatest genius among those whose signatures appear in the Society's Charter Book, but one of the greatest scientists the world has ever known. He added enormously to the prestige of the Society and also devoted much time to its administration. His successor was Sir Hans Sloane, who also had a relatively long term of office, from 1727 to 1741. Sloane was a distinguished physician and natural historian. He was, in addition, an experienced and vigorous administrator and served the Society well in this respect. Possibly, however, his greatest services were rendered earlier (1693–1712), when he held the office of secretary. These years were part of a period of exceptional financial difficulty: that they were survived is due in large measure to the devotion of Sloane, together with Sir John Hoskins, a Vice-President.

In its early days the Society had to reconcile the need to secure endowment sufficient for survival with the fact that few scientists are wealthy. There thus began the custom of electing as Fellows men of wealth and influence who often had little or no scientific attainment, and these often exceeded the scientific Fellows in number: in the latter part of the eighteenth century there were times when none of the officers was a man of science. Nevertheless, the Society was often able to find a President who combined all these desirable qualities. Such, for example, was Lord Macclesfield (1752–64), whose private observatory in Oxfordshire was equipped with the finest instruments of the day. Lord Morton, who followed him (1764–68), was also an astronomer of note: in these pages he deserves special mention as having taken an active part in the preparation for Cook's voyage in the *Endeavour* to observe the transit of Venus in 1769.

For the same reason Sir Joseph Banks is of special interest, for as a young man he accompanied Cook upon this epic voyage and was responsible for the scientific staff. But this was only the prelude to a brilliant career, which

included his being President of the Royal Society for forty-one years, by far the longest of any tenure. Banks was a wealthy man, with a deep interest in botany and agriculture. His personal friendship with George III, and his wide circle of influential friends, made him a powerful man: this, combined with his devotion to the Society's affairs, made him an outstanding President. His successor, William Hyde Wollaston, a distinguished chemist, held office for only a few months: he was followed by another great chemist, Sir Humphry Davy (1820–27).

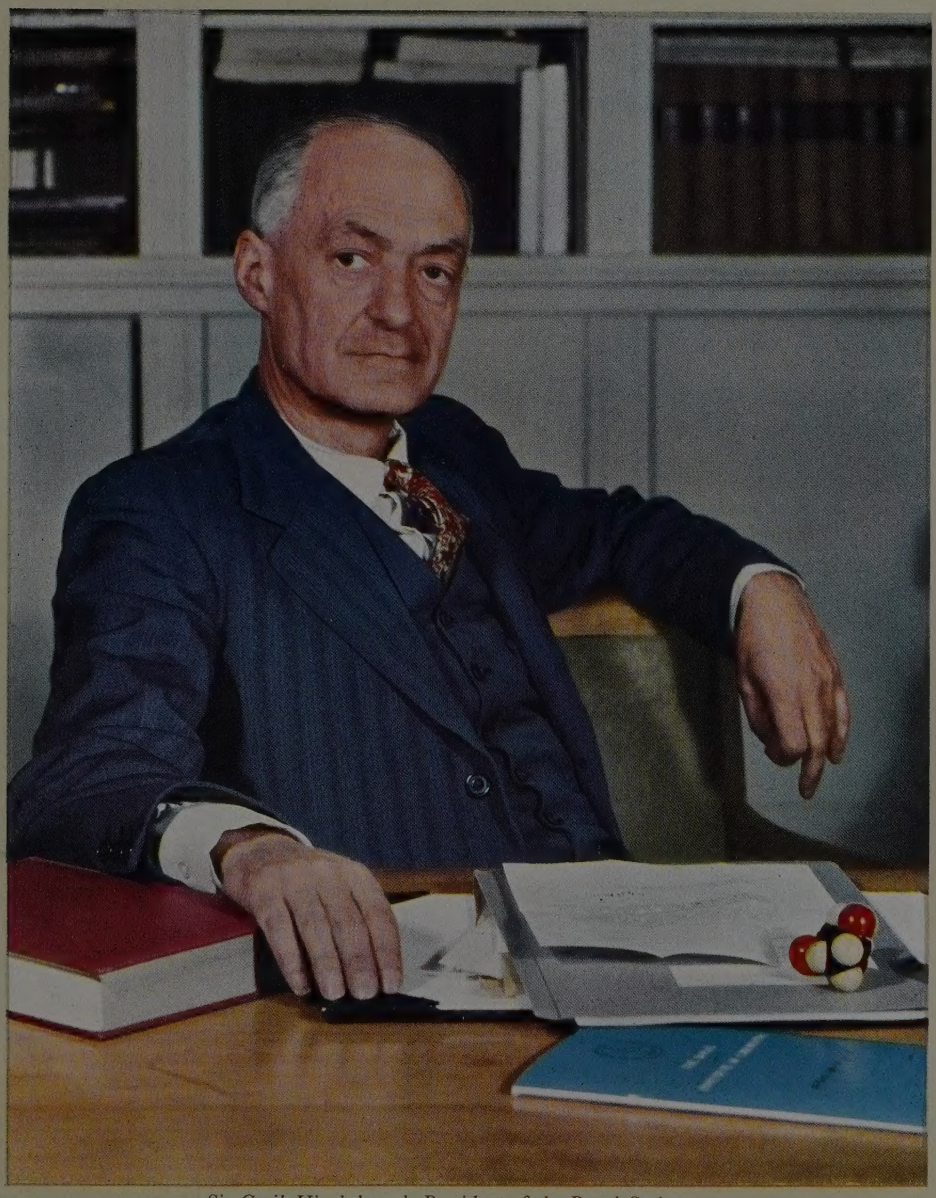
By the middle of the nineteenth century it was felt that there were some disadvantages in Presidents holding office for long periods. When Sir Joseph Hooker—one of the great directors of the Royal Gardens, Kew—was elected in 1873 he stipulated that he should not be expected to hold office for more than five years: this has since become the customary tenure. Hooker was followed by the biologist Thomas Henry Huxley (1883–85), famous for his vigorous support of the theory of evolution during a time of fierce controversy. Among his close successors was Lord Kelvin (1890–95), a distinguished mathematical physicist and also a practical man of affairs.

Of twentieth-century Presidents, many were concerned with the advance of physics. Sir William Huggins (1900–5) was a pioneer of astrophysics. Lord Rayleigh (1905–8) was one of the last of the great classical physicists and the first President to be also a Nobel Laureate. He was followed by three pioneers of atomic physics—Sir J. J. Thomson (1915–20), Lord Rutherford (1925–30), and Sir William Bragg (1935–40): the enormous influence of their researches on modern science and civilization entitles them to be counted among the giants of science. The office of President was also filled by men famous in other fields. They include Sir William Crookes (1913–15), whose many contributions include a far-sighted prophecy of the existence of isotopes, the discovery of thallium, and the spectroscopic identification of the inert gases of the atmosphere discovered by Ramsay; Sir Frederick Gowland Hopkins (1930–35), the father of biochemistry; and the pioneer physiologist of the nervous system, Sir Charles Sherrington (1920–25).

These are some of the great men of the past who have helped to shape the history of the Royal. Three past-Presidents are happily still with us and taking part in the Tercentenary celebrations. They are Sir Henry Dale (1940–45), Sir Robert Robinson (1945–50), and Lord Adrian (1950–55). All three are still actively engaged in their own fields of science, in which they long ago established international reputations. As Presidents, their task was to guide the Society through the last war and the difficult years that followed it.

The office of President has a special significance at the Tercentenary, which is to be celebrated in July by a great gathering in London of representatives of every major centre of scientific research in the world. For such an occasion the Society is fortunate indeed in its present President, for very few scientists have the breadth of interest and international associations of Sir Cyril Hinshelwood. In his now classic researches on reaction kinetics he closely linked physics with chemistry. In later work he has gone further, however, for his stimulating researches on the kinetics of bacterial growth involve the application of physical, chemical, and biological methods. His versatility in research is matched by his ability as a teacher, as all those fortunate enough to have been taught by him at Oxford can testify. Virtually the whole of his working life has been spent at, and devoted to, Oxford, where many of the meetings of philosophers that preceded the foundation of the Royal Society took place. That Oxford is today not only still a stronghold of the classics but an internationally recognized centre of scientific research is due to the perseverance of a relatively small group of scientists there, past and present, among whom Hinshelwood has long occupied a leading position.

No less noteworthy than Hinshelwood's achievements in the world of science are those outside it. He is, for example, an unusually gifted linguist with a good knowledge of French, German, Italian, Spanish, and Russian and—a rare accomplishment in the West—a working knowledge of Chinese. Moreover, he is a classical scholar of sufficient distinction to have been simultaneously President of the Classical Association and of the Royal Society—a unique attainment. He is a knowledgeable collector of Chinese porcelain and a competent painter in oils, as his rooms in Exeter College testify. While Hinshelwood is not a representative scientist, for he is too remarkable an individual to be representative of anything but himself, he is a splendid refutation of the belief—still far too commonly held—that all scientists are narrow and uncultured specialists. He is in the best tradition of Presidents of the Royal, and ENDEAVOUR is proud to salute him—as chief representative of his illustrious Society—in this Tercentenary year.



Sir Cyril Hinshelwood, President of the Royal Society.

The stellarator and other thermonuclear projects in the United States

M. B. GOTTLIEB

The preliminary work towards the harnessing of thermonuclear power has already been described in ENDEAVOUR. There are a number of possible approaches to the problem of heating and containing a totally ionized gas, and this article describes current American research on the fundamental physical processes, with special reference to the stellarator.

It was at the 1958 Atoms-for-Peace Conference at Geneva that the first full disclosure was made of the work, aimed at the controlled release of the latent fusion energy of deuterium, which had been going on in the United States since 1951. A survey of this work has been given by A. Bishop [1]. For a more detailed account the reader is referred to the Proceedings of the Conference [2]. Detailed references will here be made only to work published subsequent to this Conference. The fundamental physical ideas on the pinch effect and the preliminary results of work in Britain and the United States have been described in ENDEAVOUR [3].

The reactions of interest which utilize naturally occurring deuterium are:

$$H^2 + H^2 \rightarrow He^3 + n + 3{\cdot}3 \text{ MeV} \quad .. \quad (1)$$
$$H^2 + H^2 \rightarrow H^3 + H^1 + 4 \text{ MeV} \quad .. \quad (2)$$

where reactions (1) and (2) are roughly equal in probability. The triton (H^3) produced could then react with another deuteron

$$H^2 + H^3 \rightarrow He^4 + n + 17{\cdot}6 \text{ MeV} \quad .. \quad (3)$$

Alternatively, one could start with a mixture of deuterium and tritium and use only reaction (3), which occurs at a lower temperature. In either case, the reaction involves the very close approach of the two electrically charged nuclei. Because of electrostatic repulsion, these particles must be quite energetic in order to effect this close approach. Furthermore, because of the long-range nature of this electrostatic force, most collisions result in deflection or scattering of the particles, rather than in fusion reactions. Thus, even if one begins with a beam of mono-energetic particles, the velocities soon become random in direction and tend toward a Maxwell-Boltzmann velocity distribution, and may therefore be characterized by a temperature, which for a fusion reactor would be of the order of 10^8 °K. All atoms (even heavy atoms) are stripped of their electrons, and the resultant 'plasma' consists of a neutral assemblage of electrons and ions. The densities required for reasonable output power turn out to be about 10^{15} to 10^{16} particles cm^{-3}, about one ten-thousandth of standard atmospheric density. At such high temperatures, the mean free paths of the particles are far larger than any practical container. Any particles striking the wall are cooled. The particles must therefore be prevented from striking the walls, and the only methods for minimizing contact with the walls involve the use of magnetic fields.

The problem, then, is to make the output power greater than the losses in the system, which include (1) loss of plasma to the wall; (2) radiation losses; (3) the very large power dissipated in the coils which produce the magnetic field; (4) power losses in the conversion to electrical energy. The last two are well-understood engineering problems. The radiation losses by single electrons are also well understood. These losses are very substantial and lead to a stringent demand for elimination of impurities. The radiation due to collective motions of charged particles is still inadequately investigated. But it is the first, the problem of plasma containment, which is the most difficult and least understood at the present time.

The containment of single charged particles in a magnetic field is based on the fact that a charged particle of mass m (grams) moving with velocity v (cm/sec) perpendicular to a magnetic field of B (gauss) executes a circular orbit of radius

$$r = \frac{cmv_\perp}{qB} \text{ cm, with a frequency } f_c = \frac{qB}{2\pi m}$$

where q is the particle charge (e.m.u.).

Any velocity component parallel to the magnetic field is unaffected by the field. Thus in

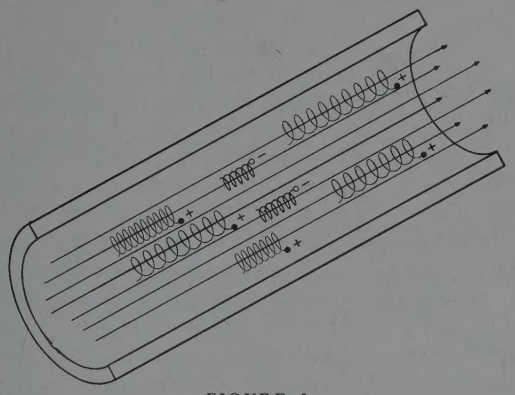


FIGURE 1

general the particles execute spiral paths about magnetic field lines, as shown in figure 1. Let us consider a long cylindrical tube containing a uniform magnetic field parallel to the axis, into which are introduced some charged particles. The particles are constrained by the magnetic field from moving out to the walls, and move freely along the field lines, and so, if one neglects for the moment the effects at the ends, the problem would appear to be solved. As the particle concentration increases, the situation becomes more complex, because the circulating charged particles affect the magnetic field and because of the collective motion of the charged particles that results from the long-range nature of electrostatic forces.

A plasma, like an ordinary gas, may be theoretically treated in terms of the position and velocity of each particle (complicated by the fact that the plasma particles are charged) or in terms of a continuous fluid characterized by such properties as density, pressure, charge density, temperature, viscosity, and dielectric constant, this being a simpler but more limited approach.

In either case the approximations are severe, and while our theoretical understanding of plasmas has made great strides in recent years, there is still a long way to go before applying most of the results to cases of laboratory interest.

The hot ions and electrons, even though constrained to move in circles, exert an outward pressure on the magnetic field of nkT (this is the familiar equation of state of a perfect gas), where n is the number of particles per cubic centimetre, k Boltzmann's constant, and T the temperature in degrees Kelvin. The action of the magnetic field on the plasma is equivalent to a pressure of $B^2/8\pi$ dynes/cm^2.

It is instructive to examine the size of some of the physical quantities. The outward pressure p in a reaction chamber at a temperature of 2×10^8 °K and density of 10^{16} ions and electrons per cm^3 would be

$$nkT = 10^{16} \cdot 1{\cdot}4 \times 10^{-16} \cdot 10^8$$
$$= 1{\cdot}4 \times 10^8 \text{ dynes/cm}^2, \text{ or about}$$
$$140 \text{ atmospheres}$$

The minimum magnetic field, B, required to contain this plasma would then be given by the equation

$$B^2/8\pi = 1{\cdot}4 \times 10^8$$

from which

$$B \sim 6 \times 10^4 \text{ gauss}$$

Thus the outward pressure of the plasma is balanced by a magnetic field pressure.

It can be shown that, if the fractional change in magnetic field seen by the particle in executing a single turn is small, the particle continues to orbit about the same lines of force, even if these lines curve or move. Indeed, the plasma and the field lines to a high degree are 'frozen' together. This is due to the fact that a plasma at high temperatures is an excellent conductor—better than copper at room temperature. Motion of the plasma pulls the magnetic field lines along with it, and vice versa. Such effects are called magneto-hydrodynamic, or hydromagnetic, effects.

However, this magnetic containment is not perfect. There are two mechanism whichs permit particles to move across even a uniform magnetic field. The first mechanism, illustrated in figure 2, is that of collisions. A particle as it moves in its orbit may collide with another, in which case it starts moving along some new circle (or spiral) displaced in a random direction by roughly one radius from the previous orbit. The effect of many such collisions is to produce a random 'walk' or drift across the magnetic field; the drift rate should be proportional to the inverse square of the magnetic field intensity. When the pressure varies

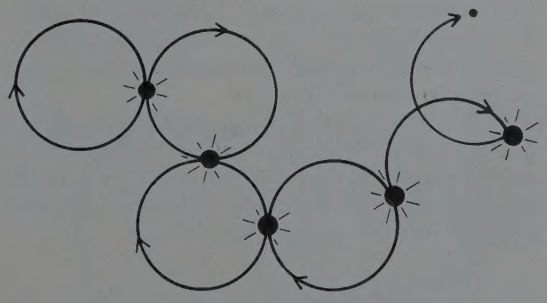
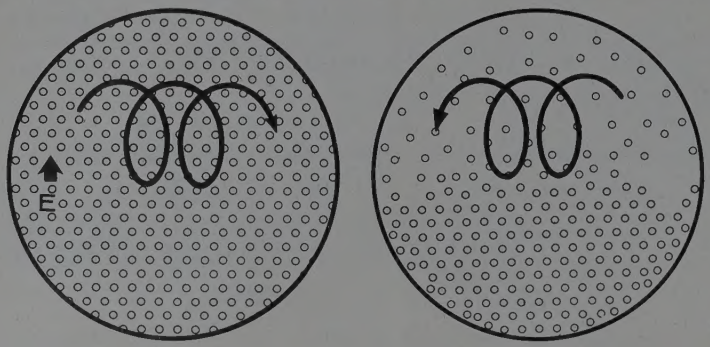


FIGURE 2



FIGURES 3 *and* 4 – *The lines of force of the magnetic field B are represented by the small circles.*

with position, there are more frequent collisions in the higher-pressure region, thus more particles diffuse from the higher to lower pressure regions than in the opposite direction. This gives rise to a velocity across the field, of magnitude $(\eta/B^2)\nabla p$, where η is the electrical conductivity, B the magnetic field, and ∇p the pressure gradient [4]. One can readily compute the energy losses due to simple particle collisions, and such losses are not serious from the standpoint of thermonuclear reactors.

Now consider the effect of an electric field perpendicular to the magnetic field, as shown in figure 3. A positive particle, as it moves up, is accelerated by the electric field E, as a result of which the radius of curvature increases gradually; however, it continues to curve and soon is moving against the electric field, thus slowing down, which results in a smaller radius of curvature. As the process continues, there results a drift perpendicular to both E and B.

Applying this argument to the case of a cylindrical plasma with co-ordinates z, r, θ (where z is the distance along the axis of the cylinder, r the distance from the axis, and θ the angle measured at the axis), with the magnetic field along the z-axis, an electric field along z causes particle acceleration along z; a radial electric field (along r) causes a rotation of the plasma; while an electric field in the θ direction causes radial motion, that is, motion towards the walls.

If the magnetic field is not constant across the particle orbit, the local curvature changes as the particle moves along. The result of this is a drift perpendicular to B and to the direction (∇B) along which B changes as shown in figure 4.

HEATING

Beside the problem of magnetic confinement of the plasma, there is also that of heating the gas to the high temperatures required for the fusion reaction. This problem is really not separable from that of confinement, for if confinement is poor this will permit energy losses. Similarly there is, as will be seen, evidence that certain heating mechanisms seem to produce disturbances which decrease the effectiveness of magnetic confinement. The heating schemes may be described in terms of two extremes. One may start with neutral deuterium gas (or deuterium-tritium mixture) at room temperature and attempt to heat this gas *in situ* by electromagnetic means, or, on the other hand, one may inject a beam of high-energy particles into the system and cause them to be scattered by some mechanism and thus come to kinetic equilibrium. Various compromises are of course possible.

THE PINCH EFFECT

One of the earliest approaches used in both the United States and Britain, and in many ways the simplest, was the pinch discharge.

If a sufficiently large current flows through the gas, the magnetic field surrounding the current may exert enough pressure to compress the current column. Compression, of course, heats the plasma, just as compression heats any ordinary gas. Thus, by means of such very large currents, both heating and magnetic containment may be effected. Unfortunately, this containment was shown both theoretically and experimentally to be unstable. If the magnetic field (or the plasma) is displaced, say in the form of a small kink, as shown in figure 5, the magnetic field lines are shifted. The magnetic field increases below and decreases above, thus tending to increase the displacement. Together, the plasma and magnetic field are driven toward the wall.

A modified system, the 'stabilized pinch', was shown to be theoretically stable. According to this concept, a magnetic field would initially be applied along the axis of the tube as shown in figure 6*a*. Then a large current would be passed through the plasma. As the discharge pinched in, the magnetic field, which would be frozen into the plasma, would be compressed as shown in figure 6*b*. The current-carrying channel with the frozen

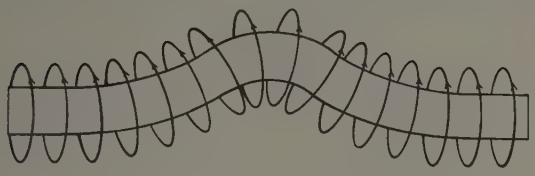


FIGURE 5

longitudinal field would be surrounded by, and separated from, the circular magnetic field lines produced by this current. Lines of force have physical properties similar to those of stretched rubber bands, and they would thus resist the tendency to kink, particularly for small wavelengths. Long wavelengths would be stabilized by the conducting walls, since the motion of lines of force into the walls would generate eddy currents such as would produce a force opposing the motion.

Unfortunately, this theory assumes infinite plasma conductivity. When consideration is given to the finiteness of the conductivity, the longitudinal and circular field lines can no longer be completely separated, and it turns out that the containment scheme is once more hydromagnetically unstable, a prediction verified by experiment.

The Los Alamos group under J. L. Tuck, and a Livermore group under S. A. Colgate, have carried out thorough and careful series of experiments showing the onset of instabilities and demonstrating that any neutrons observed are indeed due to these instabilities. Is a truly stable pinch possible? Further theoretical work has shown that it is conceptually possible for short time-intervals. However, the required magnetic field configurations are quite complex, and it is not yet known whether such configurations can actually be achieved in the laboratory.

THE MAGNETIC MIRROR

It was shown earlier that the circular motion of charged particles in a magnetic field limits their motion across the field, but allows free motion along it. Consider now the motion of a charged particle moving in the magnetic field shown in figure 7. At all times the force is perpendicular to the magnetic field. Thus, in the uniform field, the particle executes a uniform spiral progressing from left to right. When the particle moves into the region of increasing field strength, the force on the particle has a component towards the left, represented by the arrow at *F*. This decreases the horizontal component of velocity (increasing the velocity perpendicular to *B*, keeping the speed

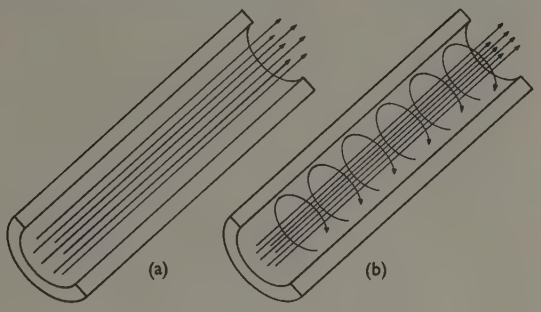


FIGURE 6

the same) and, if this force is large enough, may actually reverse the horizontal component of the velocity. This 'reflection' is the reason for the name magnetic mirror. A similar magnetic configuration may be placed at the other end. Of course, a particle moving horizontally is unaffected by the magnetic field, and escapes through the mirror. The effectiveness of the magnetic mirror depends on the ratio of the horizontal velocity to that in the plane perpendicular to that direction, that is, on the initial direction of the particle. Indeed, it may easily be shown that all particles with initial velocity (in the weak field region) lying within a cone of half-angle θ, where θ is given by

$$\sin^2\theta = \frac{B_{\text{weak}}}{B_{\text{strong}}},$$

will escape, and all others will be reflected by the mirror. Collisions between particles continue to replenish the supply of particles having velocities within the escape cone, thus providing a steady particle drain. The leakage rate has been theoretically estimated and does not seem to be prohibitively large from a fusion-reactor standpoint, particularly if the reactor were to be operated at somewhat higher temperatures than would otherwise be needed. (The collision cross section falls in proportion to the square of the reciprocal of energy.) The experimental data of G. Gibson and E. Lauer [5], and R. F. Post, R. E. Ellis, and R. Ford [6], are consistent with the

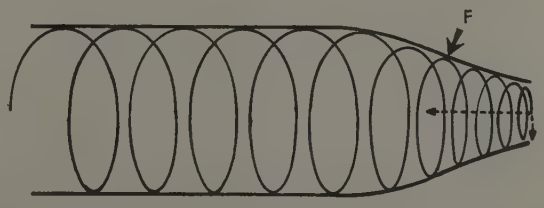


FIGURE 7

theoretical estimate, but further confirmation, particularly at higher densities and temperatures, is needed. This type of magnetic confinement is being utilized in a number of American programmes, which, however, apply different methods of heating the plasma.

At Livermore, a group under R. F. Post has worked quite extensively on the theoretical and experimental aspects of mirror confinement. Although they have made lengthy studies of various types of high-energy particle injection, most of their work has involved low-energy plasma injection, followed by compression heating [7]. The plasma is initially created by means of an arc discharge between two titanium electrodes which have been 'loaded' with deuterium gas. This plasma consists of electrons at a temperature of about a million degrees (100 eV average energy) and deuterium ions of roughly one-tenth this energy. On the time scales involved in these experiments, an insufficient number of collisions takes place between ions and electrons to bring them to the same temperature. This plasma is injected along (sometimes across) the lines of force during the time when the magnetic field is low but rising. The injection is followed by one or more stages during which the magnetic field is progressively increased. As the field increases, the lines of force move toward the axis. Since the plasma is 'tied' to the magnetic lines, it is thus compressed and heated.

In this manner, electron temperatures estimated at 10^8 °K have been reached, with an ion temperature roughly a factor of ten lower, and a particle density of about 10^{13} cm^{-3}. This hot plasma has apparently been confined for times of about 30 milliseconds. This is a result of considerable importance, because theory—rather oversimplified, it is true—predicts that the mirror confinement geometry should be hydromagnetically unstable. In this case the predicted instability is different from the kink instability applicable to the pinch type of discharge. It is termed the interchange or 'flute' instability, whereby the magnetic field and plasma move outward in one region all along the system, and inward in an adjacent region, producing growing waves, or flutes, which parallel the field lines, as shown in figure 8. This mixing might be expected to carry particles rapidly to the walls, and should get worse as the ratio (β) of particle pressure, nkT, to magnetic pressure, $B^2/8\pi$, increases. In experiments a β value of roughly 8 per cent has been reported [6], certainly high enough for one to expect to observe instability effects. It is very important to understand the reason for this apparent discrepancy, so as to know whether to anticipate these difficulties in this or other systems as one approaches the densities and temperatures required for a practical reactor.

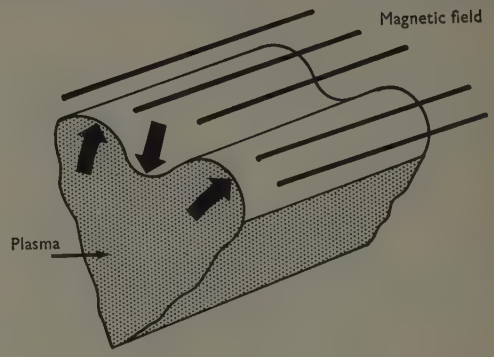


FIGURE 8

One other important problem is that of generating an initial plasma free of major impurities. About 50 per cent of the ions initially produced by the arc-discharge sources used in these experiments are heavy impurity ions. While the mirror initial-compression mechanism and the mirror loss mechanism both discriminate against the heavier and more highly ionized impurities, it is very doubtful if final impurity levels are low enough.

COLLAPSE OR THETA PINCH

This version of a mirror device has been studied at Los Alamos under J. L. Tuck, and at the Naval Research Laboratory under A. C. Kolb. It involves a mirror field created by a single turn, as shown in figure 9. The field rises very rapidly to a level of the order of 100 000 gauss, thus inducing a circumferential electric field. While the detailed mechanisms are probably quite complex, this essentially imploding magnetic field would be expected to ionize most of the gas, if at a sufficiently high pressure, and to heat the plasma to a high temperature by compression. The results appear consistent with an ion temperature of $5–10 \times 10^6$ °K [8, 9] giving a neutron flux of about 10^7 per pulse, although it seems that internal electric fields contribute to the neutron production to some degree [10].

As yet it is not clear whether, in principle, such a device could be used as the basis of a power-producing reactor. The final volume of the plasma is small compared with the volume of the

magnetic field. This means that much energy is stored in the magnetic field, and this energy would have to be efficiently recovered.

INJECTION AND THE DCX (DIRECT CURRENT EXPERIMENT) APPROACH

As an alternative to heating the gas *in situ*, there exists the possibility of accelerating ions to high energy externally and injecting these ions into the magnetic field. For example, particles could be injected longitudinally at the ends of the mirror. By symmetry it may be seen that these particles will simply escape past the opposite mirror, unless the particle makes a collision inside the chamber. For devices of any reasonable size, the mean free path between collisions is large compared with the size of the device, and most of the particles would simply escape. Transverse injection is an alternative, but after executing one circular orbit the particle would strike the injector. By shifting the injector-angle slightly so as to supply some axial velocity, one may achieve an orbit whereby the particle spirals back and forth several times before striking the injector. Thus one achieves a longer path, during which collisions may result in trapped ions.

A variation of the injection scheme involves the injection of energetic molecular ions (D_2^+) instead of atomic ions (D^+). These molecular ions, when they engage in a collision, may break up into two D^+ ions and an electron, or one D^+ ion and one neutral atom. The D^+ ion has about half the momentum of the original D_2^+ ion, and thus moves in an orbit of half the size of the D_2^+ orbit. It thus avoids the injector. The method used at the Oak Ridge National Laboratory, under P. R. Bell and E. D. Shipley, is based on the observation of J. S. Luce that a carbon arc is highly effective in breaking up molecular ions. Energetic molecular ions are injected so that they cross a carbon arc which has been formed between two electrodes and follows the magnetic field. The resultant orbits are shown in figure 10.

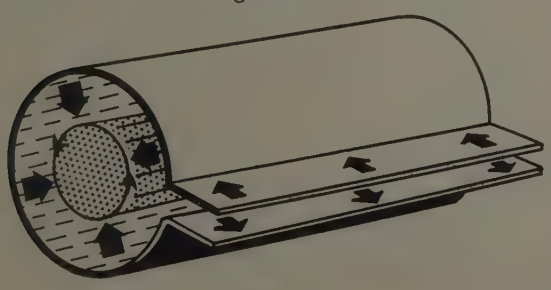


FIGURE 9

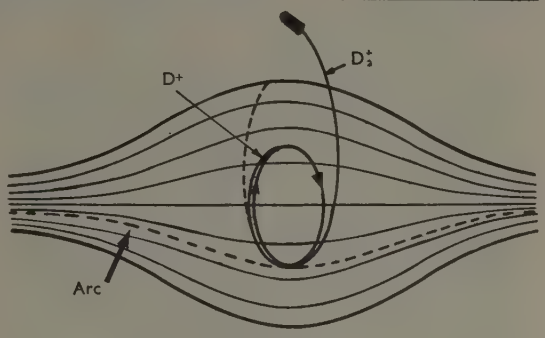


FIGURE 10

The main loss-mechanism for the trapped atomic ions is expected to be charge-exchange collisions with neutral atoms. The fast neutral atom is not confined by the magnetic field, and is lost to the walls. The major initial problem is to inject ions into the system faster than they are removed by charge exchange. Once this threshold, called 'burn-out', is reached it should in principle be possible rapidly to build up the density [11]. The Oak Ridge group has been able to demonstrate a substantial current of trapped ions in the DCX device, and they have shown that the trapped ions are confined for times of the order of 10 milliseconds.

There are a number of problems here. As yet the injection current is not large enough, but presumably this difficulty will be overcome as the techniques improve. More basic is the question of whether the fluctuating electric fields in the arc induce enhanced diffusion to the walls. Carbon ions cannot be allowed in significant numbers in a fusion reactor, so it seems likely that the carbon arc would have to be replaced by a deuterium arc, which is less efficient in achieving molecular-ion break-up. Lastly there is the question of mirror stability mentioned before.

THE STELLARATOR

The stellarator programme at Princeton University is led by Lyman Spitzer, Jr, who conceived the basic approach.

When one considers the problem of end losses from a long cylinder with a longitudinal magnetic field, an obvious solution would appear to be simply to bend the cylinder round, forming a torus. Unfortunately this simple solution does not work. The magnetic field necessarily varies as $1/R$, where R is the distance from the axis of symmetry of the torus, as shown in figure 11. As a consequence of this field variation, positive ions

drift up, electrons drift down, and a vertical electric field is created as shown in figure 12. The electrons are prevented by the magnetic field from moving to neutralize this charge accumulation. The whole plasma drifts at right angles to both the electric and magnetic fields, that is, outward from the axis. This difficulty may be circumvented by twisting the torus into a figure-8, as shown in figure 13. Consider a cross section (figure 14), taken through the tube, say at B—B of figure 13, and follow an arbitrary magnetic line of force, say at P_1, completely round the figure-8. It will now intersect the plane at a different point, P_2. As the line of force is again followed round the figure-8, points P_3, P_4 . . . are successively defined. On each successive traverse the line is found to have simply rotated through an angle i about the magnetic axis, which is the only line of force which closes on itself. Indeed, any one line other than the axis eventually (except for the unusual cases where i is an integral submultiple of 360°) traces out a surface, and the complete field may be described as a set of nested surfaces. In such a magnetic geometry, equilibrium confinement is now possible, for electrons may now neutralize charge accumulations simply by following along the lines of force. A similar, though not identical, result could be effected, as in Zeta [3], by causing a plasma current to flow about the torus, this current giving rise to a rotational component of the field. However, in this case the system is limited to pulsed operation rather than the steady-state operation visualized for a power-producing stellarator. This equilibrium of the figure-8, however, turns out to be theoretically subject to the same type of hydromagnetic instability as the mirror device. A different version of the stellarator is in theory hydromagnetically stable. In this case, instead of twisting the torus, the field lines are twisted inside the torus. This is effected by adding to the uniform windings, that provide the field along the cylinder, another helical set with reversed currents in alternate windings, one form of which is shown in figure 15. This produces a rotation of the lines of force which is proportional to r^2, where r is the radius of the cylinder. Thus successive layers of magnetic lines of force are twisted with respect to one another. These successive layers tend to prevent neighbouring layers from moving, in a manner analogous to the layers in a ball of yarn.

The procedure originally envisaged for heating the gas involved, first, a current about the torus, ionizing the gas and heating the plasma. The

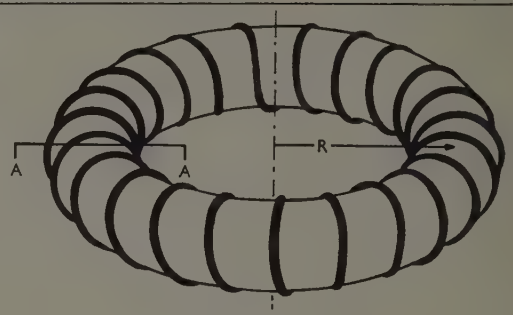


FIGURE 11

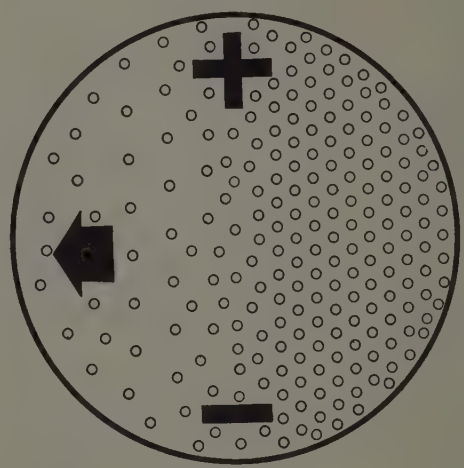


FIGURE 12

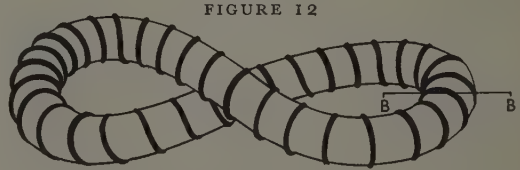


FIGURE 13

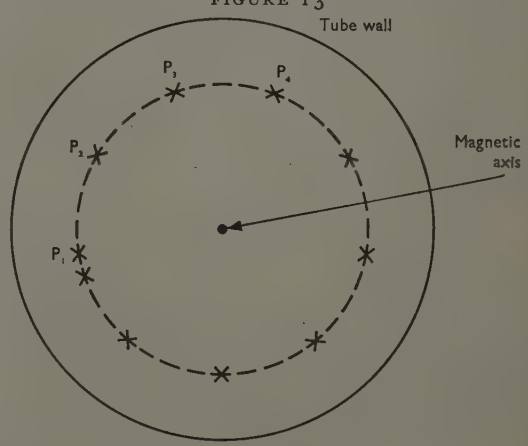


FIGURE 14

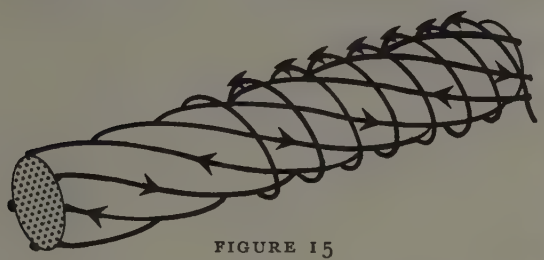


FIGURE 15

current used in this heating procedure is far smaller than that required to pinch the current channel. This current must be kept below the level (Kruskal limit) which theoretically and experimentally causes a kink instability similar to that observed in the pinch. The energy imparted by ohmic heating goes primarily into the electrons. It must be transferred from the electrons to the ions by collisions. As the plasma gets hotter the resistivity falls (as $T^{-\frac{3}{2}}$), and thus the ohmic heating should become less effective. Thus, ohmic heating should be reasonably useful only up to about a million degrees.

It is intended to use another means, called 'magnetic pumping', which gives energy directly to the ions, after the ohmic heating. The main axial confining field in a portion of the system is alternately increased and decreased, alternately compressing and expanding the plasma, and thus heating and cooling it. At first glance it would appear that no net heating results. If, however, the rate of the process is adjusted to match one of the natural frequencies of the system, net heating is possible. If, for example, the time between compression and expansion of the field over some section of the system is adjusted to be equal to the average time for an ion to traverse this section, the ions that are heated on compression will on the average be replaced by cooler ions by the time the plasma is expanded, and the heating effect will be greater than the subsequent cooling. Magnetic pumping can, in theory, heat the plasma from one million to 100 million degrees in a few milliseconds. Little work has been done on this type of magnetic pumping, because the power input to the plasma of this system is small compared with the unexpected plasma power losses during ohmic heating.

Another natural frequency of interest for magnetic pumping is that of the ions circling about the magnetic field—the ion cyclotron frequency. T. Stix has demonstrated that it is possible to transfer energy to the plasma at high efficiency using this method.

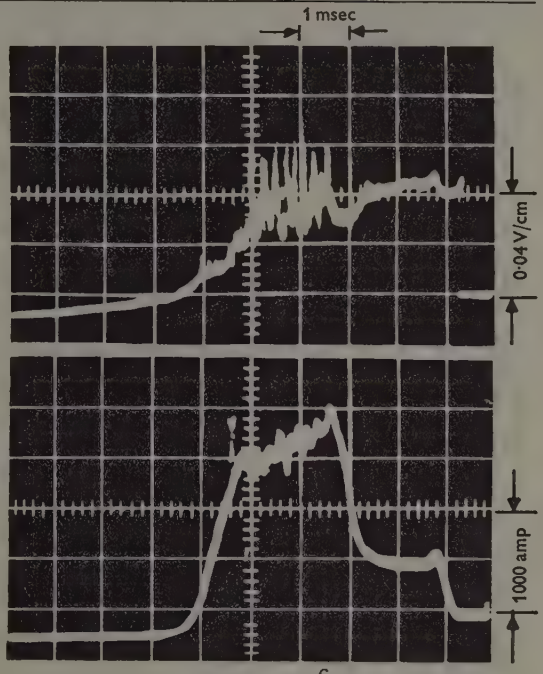


FIGURE 16

STELLARATOR EXPERIMENTS

In the B-3 stellarator the vacuum chamber is in the form of a figure-8, 680 cm in axial length and 5 cm in diameter. The peak magnetic field, 50 000 gauss, is provided by steel-encased coils. This device is operated in pulses about once a minute at high magnetic fields, and once a second at low. The magnetic field rises sinusoidally to a peak in about 0·01 sec and then falls exponentially with a time constant of about 0·01 sec. The magnetic field is within 90 per cent of the peak value for 0·01 sec, during which time the heating experiments, using helium or hydrogen, are performed. The energy for the magnetic field is supplied from a capacitor bank storing about two million joules. The electric field for ohmic heating is applied by means of a transformer.

For sufficiently high ohmic-heating fields, the kink instability is an obvious feature, as may be seen from the oscillograms in figure 16. The upper oscillogram shows the applied electric field (ohmic-heating field) as a function of time: the lower oscillogram shows the plasma current (ohmic-heating current). The current for this helium discharge rises relatively smoothly, levels off for a short time followed by a rise to a peak value, the Kruskal limit (kink instability), after which there are large fluctuations, which are also reflected in the measured electric field.

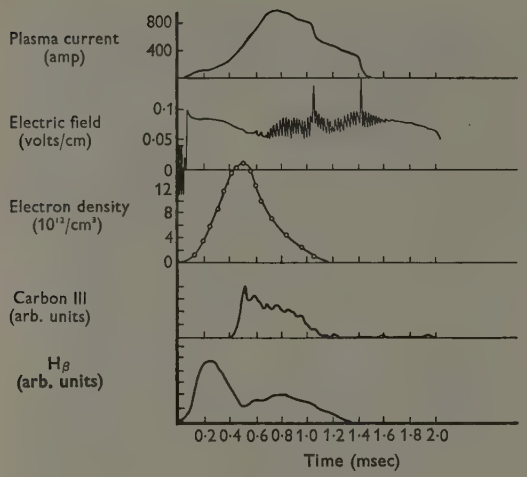


FIGURE 17

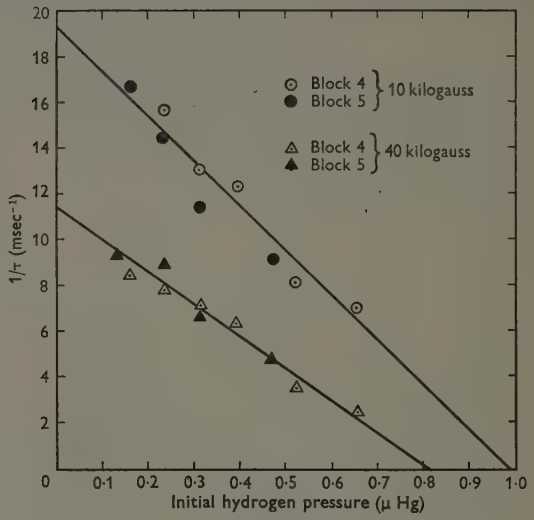


FIGURE 18

Thus, theory and experiment agree that the heating current must be kept below the Kruskal limit. Under such conditions the plasma temperature rises, roughly as predicted, to about a million degrees, but in the presence of a current there exists a serious plasma-loss mechanism. The measured characteristics of a number of physical quantities during a discharge in hydrogen are shown in figure 17. Following application of a small pre-excitation voltage at radio-frequency, the electric heating field of about 0·1 volts/cm is applied. The plasma current reaches a peak well below the Kruskal limit in about 0·6 millisecond and then drops off irregularly, while at the same time the electric field becomes quite 'noisy'. The neutral-hydrogen spectral (H_β) light, which (neglecting effects of variation of temperature) should be proportional to the neutral hydrogen and the electron concentrations, reaches a peak before the electron density does, thus demonstrating that most of the hydrogen has become ionized. At the time the electron density reaches a maximum, both the neutral hydrogen light and impurity (carbon III) light rise, indicating an influx of both impurities and neutral hydrogen into the system. After the peak, the electron density falls roughly exponentially with a time constant, τ, of about 100 microseconds.

This phenomenon of the rapid decrease of electron density, marking a loss of plasma to the walls, is called 'pump-out'. The pump-out rate depends markedly on initial hydrogen pressure, as shown in figure 18. It would appear that, roughly at the time of peak electron density, an instability arises, throwing hydrogen plasma into the wall. As a result of this wall bombardment, the neutral hydrogen atoms and the impurity atoms, both of which were loosely bound to the wall, are released into the discharge, thus both accounting for the increase in the light emission and—as these neutral atoms become ionized—tending to maintain the electron density. One can account for the slower pump-out rates under the conditions of higher initial gas pressure by assuming that the amount of neutral gas bound loosely to the walls is proportional to the initial pressure. This theory tends to be supported by the observation that pump-out is much slower in helium, which, being chemically more inert, would have less tendency to stick to the walls. Presumably helium ions become neutralized at the walls and then diffuse back into the discharge. Indeed, pump-out was not experimentally observed until, in an attempt to reduce the effects of impurity radiation, the original vacuum systems were replaced by ultra-high-vacuum systems with base pressures of the order of 10^{-10} mm Hg. This improvement of a factor of 10^4 over ordinary high-vacuum technique results in much cleaner walls.

If this physical picture is correct, the pump-out time, extrapolated to zero pressure, would represent the plasma loss rate unaffected by the introduction of new material. It is roughly a factor of 1000 more rapid than would be expected on the basis of collision effects. This extrapolated pump-out time shows a slow variation with the magnetic

confining field (varying as $1/B^{\frac{1}{2}}$), whereas, on the basis of collisions alone, a $1/B^2$ variation would be expected.

This pump-out process is found to take place both in the simple figure-8 geometry and in the system with helical stabilizing windings [12], which indicates that pump-out is not due to hydromagnetic instabilities. A mechanism which would account for such losses would involve local electric fields associated with waves or oscillations in the plasma. Probe measurements indicate that electric fields do exist in sufficient magnitude to account for these losses. Under what conditions are such electric fields generated in a plasma? In the presence of an applied electric field, a small fraction of electrons travel many mean free paths without being slowed by a collision. These electrons are being continually accelerated by the field and become less and less likely to engage in a collision, and soon constitute a high-energy electron beam circulating about the torus.

The fraction of electrons which become 'runaway' electrons increases as the electric field is increased and as the electron density decreases. It is well known that an energetic beam can set up plasma oscillations. Indeed, it is this mechanism to which is attributed the large noise generation that is observed in the measured electric field shown in figure 16. Note that the oscillations start at a time when the plasma density is decreasing, which would make runaway electrons more likely. It has recently been shown [13] that even small electron-drift velocities are theoretically capable of setting up plasma waves.

The existence of pump-out does not necessarily mean that ohmic heating would not be useful. If the rate becomes smaller as the dimensions of the chamber increase, and if the instabilities associated with the current die out after the current pulse, ohmic heating would still be useful more or less as was originally intended.

In conclusion, it may be stated that neither theory nor experiment has yet demonstrated whether or not a fusion reactor is possible. It will probably take some years to acquire this information. Meanwhile one may confidently expect a marked increase in our basic knowledge about the physical processes taking place in plasmas.

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The Royal Society and its foreign relations

E. N. DA C. ANDRADE and D. C. MARTIN

In July 1960 the Royal Society of London will be celebrating the three-hundredth anniversary of its foundation. The Tercentenary is to be marked by a great gathering in London of representatives of the world's principal centres of scientific research. On such an internationally significant occasion it is timely to recall that from its very beginning one of the chief concerns of the Society has been to maintain relations with men of science throughout the world.

In his 'Defence of the Royal Society,' published in 1678, John Wallis, the great mathematician who was prominently associated with its foundation, tells that, about 1645, sundry ingenious gentlemen, keenly interested in the new experimental philosophy, met together by arrangement weekly in London to discourse on such matters as the circulation of the blood (first announced by Harvey in 1628); the nature of comets and other astronomical matters, including the improvement of telescopes; physical experiments such as the Torricellian production of a vacuum; and many other aspects of the new learning, as it may be called. A few years later some of these men moved to Oxford, where with others they pursued together their favoured studies, which did not then form any part of the official university courses, the old Aristotelian philosophy being traditional in the schools. John Wilkins, who had already written a good popular account of Copernican astronomy, was a leader of this Oxford group: he was Warden of Wadham College and arranged in due course for the ardent innovators to meet at his rooms there. Prominent among them were John Wallis; Robert Boyle; Christopher Wren, at this time a young man mainly interested in scientific study who at the age of twenty-four became professor of astronomy at Gresham College, and his uncle Matthew Wren, a prominent churchman; William Petty, political economist, administrator, and inventor; the medical men Jonathan Goddard, Thomas Willis, and John Bathurst; Seth Ward, mathematician and astronomer, who afterwards became Bishop of Exeter; and Lawrence Rooke, astronomer. Wilkins himself later became Bishop of Chester, and churchmen were prominent in the group from which sprang the Royal Society, for such it was. The Church offered at the time a widely honoured and lucrative career, which attracted a goodly proportion of the learned of the two English universities, but nevertheless it may seem remarkable that it was strongly represented among those scientific innovators. Noteworthy, too, is that when, shortly after its foundation, the Royal Society was vigorously attacked by certain professed adherents of the traditional learning, the men who most vigorously took up the pen on its behalf were churchmen, in particular Joseph Glanvill, rector of Bath, and Thomas Sprat, later Bishop of Rochester, whose 'History of the Royal Society', first published in 1667, is an account of the foundation, objects, and early achievements of the Society and a defence against disparagers.

About 1658, the chief members of the Oxford Philosophical Society—for that is the name adopted by the group, although it was frequently styled the 'Invisible College'—came to London, where they were reinforced by others who shared their interests, among whom Lord Brouncker, a notable mathematician, was one of the most considerable. Plans for the foundation of a Scientific College were rife, and during the troubled times that followed the death of Oliver Cromwell there was a very active concern with such schemes, not only in quarters where it might be expected, but among the literate in general. An instance of this is Abraham Cowley's detailed 'A Proposition for the Advancement of Experimental Philosophy', published in 1661 but clearly the result of long consideration. The Restoration, with its healing effect, gave occasion for definite action, and at a meeting of the 'philosophers' at Gresham College on 28th November 1660, to hear a lecture by Christopher Wren, it was decided to regularize the meetings by forming a Society. Meeting place, meeting times, subscriptions, treasurer, and register were decided; Wilkins was appointed to the

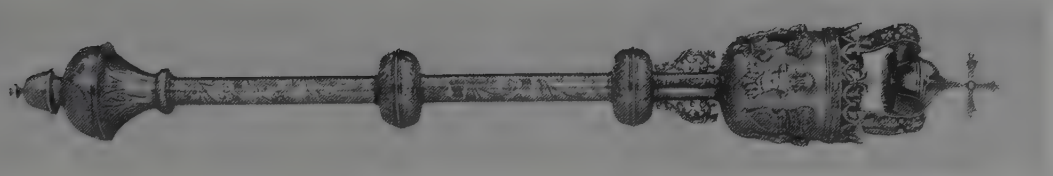


FIGURE 1 – *The mace of the Royal Society, presented by Charles II.* *(Figures 1–6 are reproduced by permission of the Royal Society.)*

chair; and a list of forty names, as members, drawn up. This may be taken to be the foundation of the Royal Society, although the name was not used, and it is the tercentenary of this date that is being celebrated this year by the Royal Society. A week later Sir Robert Moray, equally a friend of King Charles II and of the new Society, brought word that the King was ready to encourage the project. At a subsequent meeting, on 12th December, rules were drawn up governing such matters as election of members and officers of 'this Society'. By the good offices of Sir Robert Moray, a Charter of Incorporation was granted by the King in 1662, in which Charter the Society is formally named The Royal Society, or rather, since the Charter is in Latin, *Regalis Societas*. This date is sometimes taken as that of the foundation and, in fact, the two hundred and fiftieth anniversary celebrations were held in 1912.

An aspect of the Royal Society which seems particularly worthy of attention in this year of celebrations, when so many distinguished representatives of foreign academies, universities, and other institutions will be welcomed, is the relationship of the Society with other countries. From the beginning, the connection with men of science of other lands has been close and most cordial. Sprat, in his 'History' published in 1667, to which reference has already been made, considers, in assessing the encouragement that the Society had received, the intimacy and correspondence of the Society with other lands. Of France he observes that the Society has maintained a 'perpetual intercourse' with her leading physicians, travellers, mathematicians, and chemists and that it has been 'affectionately invited' to correspondence with the French Academy. Of Italy, that the Society has been invited to exchange knowledge with the leading minds, but especially with Prince Leopold of Tuscany, who was the patron of the *Accademia del Cimento*, that great association of experimenters. He says that he particularly notes their invitation 'because it comes from that Country, which is seldome wont to have any great regard, to the *Arts* of these *Nations*, that lye on this side of their mountains'. Of Germany and its neighbouring kingdoms he records that the Society has met with great veneration and refers to instruments that have been presented to the Society from that country, saying of their scientific inquiries 'For which kinds of Enterprises the temper of the *German* Nation, is admirably fit, both in respect of their peculiar dexterity in all sorts of manual *Arts*, and also in regard of the plain, and unaffected sincerity of their *manners* . . .'. In connection with the Low Countries he refers with particular praise to Huygens, saying that correspondence with him and many others is still maintained in spite of the state of war prevailing between England and Holland, which, as will be seen, is typical of the relations between the Society and foreign men of science. Christiaan Huygens had visited London in 1661, when he was warmly received by the incipient Society: when he again came to England in 1663, the year in which the first list of Fellows of the incorporated Society was drawn up, with Lord Brouncker as President, he was elected a Fellow, as was the Frenchman Sorbière.

Another name in the first list of Fellows which deserves mention in connection with the Society's relations overseas is John Winthrop, colonial governor of Connecticut, who, born in England, had settled in America. From 1661 to 1663 he was in England on a mission, successful in its outcome, to obtain a charter. He had a scientific mind, contributed to the 'Philosophical Transactions', and presented many curiosities to the Society's collection. A great-grandnephew of his, of the same name, was likewise a Fellow and published many papers in the 'Transactions', mainly on astronomy. He was, in fact, America's first astronomer of note.

Among the Fellows elected during the first twenty years figured Johannes Hevelius, the Danzig astronomer; Adrien Auzout, French mathematician and astronomer; Nicholas Mercator (whose real name, duly latinized, was Kaufmann), a celebrated German mathematician who lived in England for some time; Marcello Malpighi the

FIGURE 2 – *Henry Oldenburg*, 1626–77; *Secretary* 1663–77. *By John van Cleef.*

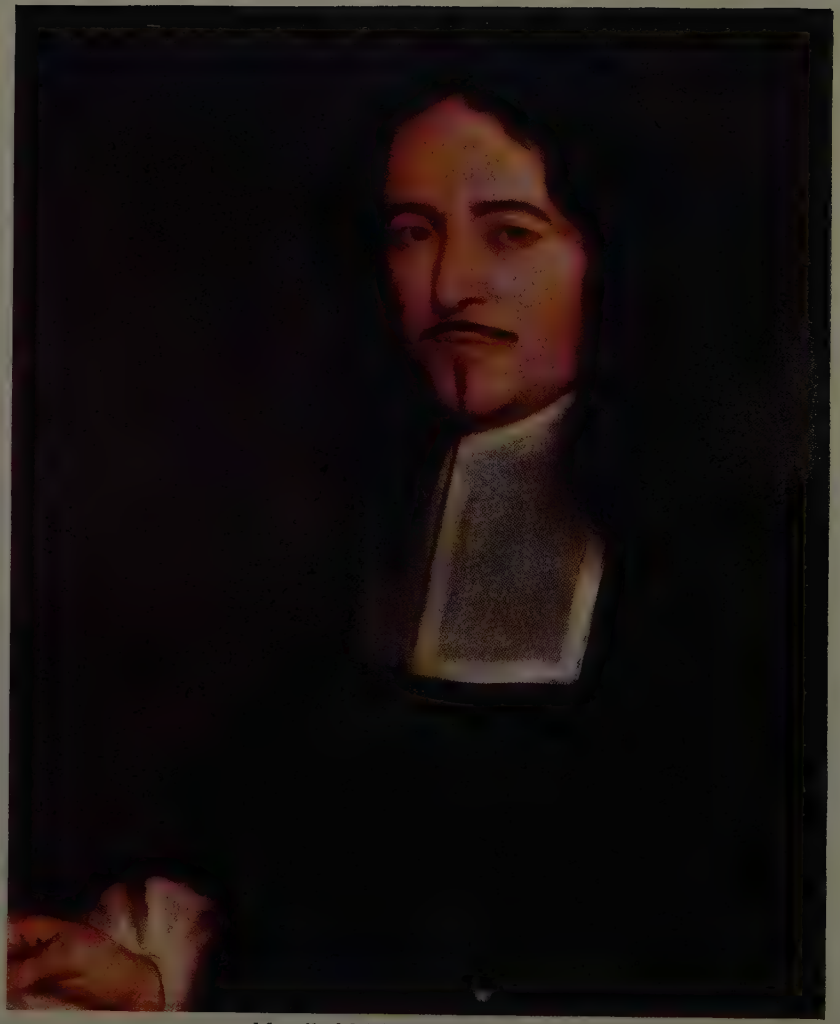
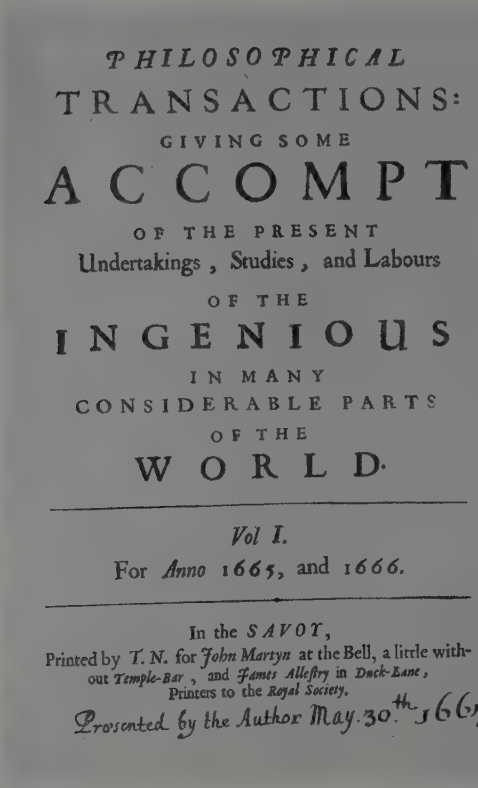


FIGURE 3 – *Marcello Malpighi*, 1628–94. *By A. M. de Tobar.*

FIGURE 4 – *Bernard de Fontenelle*, 1657–1757. *By H. Rigaud.*

FIGURE 5 – *Benjamin Franklin*, 1706–90. *By Joseph Wright.*



PHILOSOPHICAL
TRANSACTIONS:
GIVING SOME
ACCOMPT
OF THE PRESENT
Undertakings, Studies, and Labours
OF THE
INGENIOUS
IN MANY
CONSIDERABLE PARTS
OF THE
WORLD.

Vol I.
For Anno 1665, and 1666.

In the SAVOY,
Printed by T. N. for John Martyn at the Bell, a little without Temple-Bar, and James Allestry in Duck-Lane, Printers to the Royal Society.
Presented by the Author May. 30th. 1667.

FIGURE 6 – *Title page of the first volume of the 'Philosophical Transactions'.*

Italian, who may be considered the founder of microscopic anatomy; Jean Dominique Cassini, the famous astronomer who directed the *Observatoire Royal* in Paris; and the great Dutch microscopist Leeuwenhoek. Malpighi sent his famous book on the silkworm, *Dissertatio Epistolica de Bombyce*, to the Society, with the request that it should be published under its auspices; and it duly appeared, so approved, in London in 1669. Two other works of Malpighi were likewise published in London in the same circumstances. He presented his portrait to the Society in 1680, and it still adorns the walls of the Society's apartments (figure 3). Two of Denis Papin's best-known writings appeared under the Society's patronage. Leibniz in 1671 dedicated his *Hypothesis Physica Nova* to the Society, and many other foreigners paid the Society a like compliment.

From 1673 until his death in 1723 Leeuwenhoek communicated all his microscopic discoveries to the Society in the form of letters, which were duly published in the 'Philosophical Transactions'. When, in 1679, he was elected a Fellow he wrote saying that he would strive with all his might and main to make himself worthy of this honour and privilege. In his will he left the Society a cabinet containing twenty-six of his famous single-lens microscopes.

The active correspondence with foreign men of science in the early years of the Society owed much to Henry Oldenburg, the first Secretary (figure 2). He was a native of Bremen and a good linguist who was in close touch with the learned of many lands. He was not altogether a pleasant character, having deliberately aggravated trouble between Newton and Hooke, but he rendered most valuable services to the Society. He was responsible for the first appearance of the 'Philosophical Transactions' for 1665 (figure 6) and acted as editor and publisher for many years. In those days the 'Transactions' contained—besides original contributions, some of prime importance, by British and foreign authors—accounts of work being done abroad, often adapted from the *Journal des Sçavans*, and reviews of European works on a wide variety of subjects.

An English book that, in the early days, attracted much attention abroad and added to the fame of the Society was Robert Hooke's *Micrographia* (1665), which, besides accounts of fundamental microscopic observations, contained capital discoveries and inventions in many fields. A lengthy and eulogistic review appeared in the *Journal des Sçavans*, and continental men of science, such as J. C. Sturm, wrote of it in the highest terms. When in 1679 the 'Philosophical Transactions' ceased to appear, Hooke produced a scientific periodical entitled 'Philosophical Collections', which devoted most of its attention to what was being done abroad. This continued until the 'Transactions' resumed publication in 1683, since when they have appeared regularly.

A fresh advance in the Society's fame at home and abroad was occasioned by the achievements of Isaac Newton. The publication in the 'Transactions' in 1672 of his pioneering work with the prism and of a further fundamental paper on light in 1675 aroused wide interest—and much misunderstanding—but it was the appearance of the *Philosophiae Naturalis Principia Mathematica*, usually known as the *Principia*, in 1687 that established him as an outstanding genius. Emphatically the book was not understood at once, but that a mighty mind had produced it was realized abroad, especially in France. In 1714 Fontenelle (figure 4) wrote to Newton, in thanking him for a copy

of the second edition of the work, '*Il y a déjà plusieurs années que cet excellent ouvrage est admiré dans toute l'Europe, et principalement en France, où l'on sait bien connaître le mérite étranger.*' The Marquis de l'Hôpital, famed as a mathematician, spoke of Newton as a celestial genius; Leibniz eulogized him as the supreme mathematician; Voltaire made Newton's name and achievements known outside the scientific aristocracy in France by his popular book *Elémens de la philosophie de Neuton*, first published in 1738. Algarotti did the same in Italy with his *Il Newtonianismo per le Dame ovvero Dialoghi sopra la Luce e i Colori* (1737). Both books were widely translated. This glory of Newton redounded greatly to the fame of the Royal Society, of which he was President from 1703 to his death in 1727, for his name, and the achievements of British science in general, were closely linked with the Society. Among the foreign Fellows elected during Newton's reign were Magalotti, Jean and Nicholas Bernoulli, Pierre Varignon, 's Gravesande, and J. B. Morgagni. Voltaire was elected in 1743.

After the death of Newton the power and influence of the Royal Society not unnaturally suffered a decline. The man who succeeded him as President was Hans Sloane, a physician and a great collector: the treasures that he amassed formed the basis of the British Museum. Typical of Sloane was that in 1725, hearing that a youthful visitor from America had a purse made of asbestos, at that time a rarity, he purchased it and, incidentally, showed the youngster his collection. The lad was Benjamin Franklin (figure 5), and he was destined to strengthen the connection of America —at that time a British colony—with the Royal Society. After his return to America he carried out a classical series of experiments on frictional electricity, one result of which was the conclusion that lightning and the electric spark were of the same nature. An account of the experiments was first made public in a paper read by William Watson at the Royal Society, and there followed a book by Franklin, published in London, consisting of the letters describing his electrical experiments which he had sent to Peter Collinson, F.R.S., a book which had a great success. In 1753 Franklin was awarded the Copley Medal, the highest award that the Royal Society bestows: during the past hundred years it has frequently been assigned to an American.

In 1757 Franklin again visited England, this time not as an unknown youth but as a man of high renown. His relations with the Royal Society, of which he had already been elected a Fellow, were intimate and cordial. He became a Member of Council and a prominent member of several important committees, as well as a frequent guest at the dining club which existed, and still exists, within the Society. In 1762 he returned to America, but came back to England in 1764, when he resumed his intimate relations with the Society. He left England for the last time in 1775, in which year the war broke out between America and England. It is, however, typical of the attitude of the Royal Society that Franklin's views on lightning were warmly supported by its Fellows in controversies that arose during the war and that, when gold medals were struck in honour of James Cook, Sir Joseph Banks, at the time President, sent one to Franklin, with a most courteous and cordial letter. The relations of the Society with American science were, then, not affected by the war and have continued to be of the warmest ever since.

Hans Sloane was succeeded as President by Martin Folkes, an antiquary of no great accomplishment, and the performance and reputation of the Society continued to decline. In fact in the years from 1727 to 1766 no outstanding Englishman was elected a Fellow, but among foreign Fellows admitted were Buffon, Linnaeus, d'Alembert, Pieter van Musschenbroek, and Réaumur. In 1778 Joseph Banks became President, and it so happened that about this time British science was beginning a fresh period of prosperity with which the names of Joseph Priestley, John Hunter, William Herschel (a German by birth), Smithson Tennant, W. H. Wollaston, and Thomas Young are connected.

Banks ruled over the Society as President for a far longer time than any other, namely for forty-one years, the next-longest reign being the twenty-three years of Newton. He was a rich man who was passionately attached to the study of botany and formed a notable collection of specimens relating to that science. He was, however, no great originator in science and is chiefly remembered for his character and influence. As a young man he accompanied James Cook in his famous voyage in the *Endeavour*, a picture of which adorns the cover of this periodical, having organized and equipped at his own expense a staff for the collection and classification of botanical and other natural objects. As a personal friend of King George III, he was able to do much to promote the good of the Society and the national interest in science. Under his vigorous and personal direction the Society rose again to a position of great

influence and reputation throughout Europe. Banks, a widely travelled man, was particularly concerned to promote feelings of comradeship between British and foreign men of science. When the Napoleonic wars came in 1797, Banks did everything in his power to preserve friendly relations between English and French men of science. Cuvier and Lacépède were elected Fellows in 1806; Biot and Gay-Lussac in 1815, two months before Waterloo.

It is worthy of mention, too, that in 1813 Humphry Davy, who was to become President in the year that Joseph Banks died, travelled in France by special permission of Napoleon and was most hospitably entertained in Paris by the great French men of science of the time, who included Ampère, Gay-Lussac, and Cuvier.

Cuvier, in the posthumous *éloge* of Joseph Banks which he pronounced in 1821, paid particular tribute to the way in which he had protected French men of science during the war, saying '*Pendant cette guerre de vingt-deux ans qui a porté ses ravages sur presque tous les points des deux mondes, par-tout le nom de M. Banks a été un palladium pour ceux de nos compatriotes qui se livrent à des recherches utiles: si leurs collections étaient enlevées, il suffisait qu'ils s'addressaient à lui pour qu'elles furent rendues; si leur personne était détenue, le temps de lui faire parvenir leur réclamation était le seul délai qu'éprouvât leur mise en liberté.*'

An early example of international scientific co-operation also characterized the rule of Banks. In 1783 the *Académie Royale des Sciences* asked the Royal Society to co-operate in a survey to connect the Greenwich Observatory with the *Observatoire Royal* in Paris. A grant was obtained by the society from King George III for the expenses of this work, and the triangulation was duly carried out by the two countries in 1787.

After the death of Banks, the cordial relations with foreign countries continued, the great investigators of all lands being elected to the Society. With them a number of lesser men had been admitted in the middle of the eighteenth century. To remedy this, in 1787 the total number of Foreign Members was limited to a hundred, and about this time the present difference of nomenclature became standardized, foreigners who belonged to the Society being called Members, as distinct from Fellows, the term applied to British subjects. In 1825 the number of Foreign Members was further restricted to fifty. It may be remarked that in 1945 the total number ceased to be restricted, but the annual elections were limited to four. Today the total number stands at 61.

During the world war of 1914–18, although conditions forbade intercourse between British and German men of science, all the German Foreign Members were retained, and the same was the case, in spite of considerable provocation from some of the political extremists of the German scientific world, in the last world war. The Society was delighted that, at its special invitation, Max Planck, a universally honoured figure, was present at the Newton celebrations held just after the war, in 1946, and the election of German Foreign Members started again with that of Max von Laue in 1949.

We now turn to the organized international co-operation in science, in which the Royal Society has always played a leading part. Co-operation of a formal kind between academies of science can be said to have begun in 1898, although there had previously been international co-operation between scientists, for example in the International Geodetic Association, which derived from the International Mid-European Association set up in Berlin in 1861; in the *Bureau International des Poids et Mesures* established in 1873; and in the First Polar Year in 1882–83. It was in 1898, however, that a group of academies in Germany invited the Royal Society to join them at a meeting, the outcome of which was a proposal, put forward by the Society, to form an International Association of Academies. Within the year the French, Russian, and United States Academies had agreed to a meeting at Wiesbaden, at which in October 1899 a scheme for a General Assembly of Academies was approved. Each Academy was to send delegates to the triennial meeting of the Assembly, which would be in two sections, one dealing with the natural sciences, the other with 'the sciences of language, history, philosophy and antiquities, and other subjects, the study of which is based on scientific principles but which are not included under the term "Natural Science".' The first formal meeting of the International Association of Academies took place in Paris in 1900.

It was this new international action that led directly to the formation of the British Academy. The Royal Society was the obvious body to represent Great Britain in the natural science section. It was invited to widen the scope of its interests so as to qualify for admission to the other section, but the Fellows at a Special Meeting in May 1901 showed themselves to be not in favour of the proposal and the Council accordingly decided against it. In the discussions that preceded

this decision many distinguished representatives of historical, philosophical, and philological learning had been invited to take part, and, with the encouragement of the Society, these representatives petitioned the King for a new chartered body. In August 1902 a Grant of a Royal Charter was made and 'The British Academy for the Promotion of Historical, Philosophical and Philological Studies' came into being. The Royal Society gave the new body substantial encouragement, and until 1928, when the Academy acquired accommodation of its own, it met in the Society's rooms.

The International Association continued until the 1914–18 war made it impossible to hold the General Assemblies, but in 1918 the Royal Society took steps to revive international co-operation in science. A meeting took place in the Society's rooms at Burlington House in October 1918, followed a month later, immediately after the Armistice, by another meeting in Paris, where an executive committee was commissioned to make preparations for a new International Research Council. This, with the help of a number of affiliated international Unions, was to be a means to further international scientific relations. The first General Assembly of the International Research Council took place in Brussels in July 1919, when Sir Arthur Schuster, Foreign Secretary of the Royal Society, was elected General Secretary of the Council. The Royal Society was appointed the British adhering body to the International Research Council and its Unions.

The first five General Assemblies were all held in Brussels between 1919 and 1931. In 1928, Sir Henry Lyons, an able and enthusiastic supporter of international scientific activities, became Foreign Secretary of the Royal Society and also became General Secretary of the International Research Council. The Royal Society throughout this period gave this Council very strong support.

At the fifth General Assembly in 1931, constitutional changes proposed by the Royal Society were adopted. This resulted in the name of the Council being changed to the International Council of Scientific Unions (ICSU). Sir Henry Lyons remained as General Secretary of ICSU. With the outbreak of war in 1939, the work of ICSU and its member Unions was, of course, interrupted, but as early as 1943 the Royal Society had already begun to encourage the revival and reconstruction of international scientific relations and was in communication with the Soviet and American Academies of Science with this in view. In July 1946 the Fourth Assembly of ICSU took place in the Society's rooms. Succeeding General Assemblies of ICSU were held in Copenhagen (1949), Amsterdam (1952), Oslo (1955), and Washington (1958). After the Amsterdam meeting Professor A. V. Hill succeeded Professor Stratton as General Secretary and later Sir Harold Spencer Jones succeeded Professor A. V. Hill. Since the inception of the International Research Council in 1918, therefore, until 1958, all the General Secretaries of the Council have been Fellows of the Royal Society.

The work of ICSU and, in particular, the organization of the International Geophysical Year (IGY), for which it was responsible, was described in an article in this journal by Sir Harold Spencer Jones a year ago [ENDEAVOUR, XVIII, 88, 1959].

ICSU has now created on the IGY pattern international committees for space research, for oceanic research, and for research in Antarctica. The next General Assembly is to be held in London in 1961, when the Royal Society will once again be a most willing host to a body for whose advancement it has done so much in the past forty years.

At the end of the last world war, a conference of Ministers of Education of the allied countries took place in London to consider the creation of an international organization to promote educational and cultural activities, in furtherance of the aspirations expressed in the Charter of the United Nations. As a result of discussions in which the representatives of the Royal Society took a leading part, the United Nations Educational and Cultural Organization, first known as Uneco but ultimately as Unesco, was set up, which included science in its beneficent activities. Sir Julian Huxley was the first Director General and Dr J. Needham the first Director of the Division of Natural Sciences, both being Fellows of the Royal Society. The Society now has a number of advisory panels which lend the support of selected British men of science to many of the projects of Unesco.

In addition, the Society was a promoter of the scheme to publish the 'International Catalogue of Scientific Papers' from 1900 to 1922, and it took an active part in the establishment and support of international research stations, such as the Jungfraujoch High Altitude Research Station for research on cosmic rays, high-altitude physiology, and allied matters, and the marine biological stations at Naples and at Bermuda. When the setting up of a European Nuclear Research Station

(*Conseil Européen pour la Recherche Nucléaire*) was being considered, the Society took the initiative in urging British participation. In 1928 the Society joined the Pacific Science Association and has been an active member ever since. The Society has taken a leading part in many international expeditionary activities, thereby maintaining the tradition which Edmond Halley and James Cook did so much to establish. The Society took a significant part in the expeditions connected with the Second Polar Year (1932–33) in the Arctic, and with the IGY in the Antarctic.

The Society has always had cordial relations with the British Commonwealth, all subjects of which are eligible for election to the Fellowship, and since the Second World War this co-operation with Commonwealth countries has been particularly close. In June 1946 representatives came to the Society from all the Commonwealth countries to participate in the Royal Society Empire Scientific Conference. This was followed immediately by the Newton Tercentenary Celebrations of 1946, which were attended by representatives from all over the world. In this way the Society was able to renew international contacts after the estrangement enforced by wartime conditions. In 1948 the Society held a Scientific Information Conference, mainly on a Commonwealth basis but with representatives from the U.S.A. and Unesco.

The life and work of Lord Rutherford re an outstanding example of the unity of science within the British Commonwealth. He was born in New Zealand and did his first research there; after outstanding work at Cambridge he held his first professorship in Canada, his work there earning him the Nobel Prize; he was professor at Manchester, where he first put forward the nuclear structure of the atom; his final post was the Cavendish Professorship at Cambridge, where he and his school achieved nuclear disintegration. In 1950–51 the Royal Society organized the raising of the Rutherford Memorial Fund and decided to devote the income from this fund to furthering scientific co-operation within the Commonwealth. Part of this income is devoted to the award of scholarships to graduates who have taken a first degree at a university within the British Commonwealth or Eire, the scholars being required to pursue research at a university in some part of the Commonwealth other than that where they graduated. Another part of the fund is devoted to the Rutherford Memorial Lecture, which has to be given in some part of the Commonwealth overseas.

Relations with the Commonwealth were further strengthened when in 1953 the Society created the Commonwealth Bursaries Scheme, in which foundation they received the full co-operation of, and substantial financial aid from, the Nuffield Foundation. The scheme receives the active support, including financial contributions, of the various Commonwealth scientific organizations.

In some respects the Society has been an example, and a sponsor, to several academies of science. Just as Benjamin Franklin, in 1743, when founding the American Philosophical Society 'of Virtuosi or ingenious men' had the Royal Society very much in mind as a model, so in 1951 the Fellows of the Royal Society resident in Australia formed the Australian Academy of Science on a pattern similar to that of the Society. When the Australian Academy received its Royal Charter from the hands of Her Majesty the Queen on 15th February 1954 in Canberra, its President cabled to the President of the Royal Society 'We are grateful for the encouragement and help which the Royal Society has given to the National Academy, founded by the Queen and modelled closely upon the ancient foundation to which we send our thanks and greeting.'

The Australian Academy and the American Philosophical Society will be among those academies, young and old, which in July 1960 are sending representatives to the Royal Society's Tercentenary Celebrations in London, when once again the Society will, as a host, warmly further the friendship which has characterized its international relations for three hundred years.

It is inevitable, and in accordance with the spirit of the times, that in its international relations the Royal Society should today co-operate with great corporations. The personal relations between men of science from overseas and the Society, represented by its Officers and Fellows as individuals, are, fortunately, every whit as strong now as they were in the early days, when a visit to the Society was the first concern of any scientific traveller from abroad. The Society still welcomes at its discussions noted foreign experts on the subject under consideration; it still takes the greatest pleasure in offering, through its Fellows, private hospitality as well as public praise to honoured visitors from all parts of the world. It is the confident belief of all concerned that the Society will continue to represent British science worthily and that it will always consider as a foremost duty and pleasure the maintenance of cordial relations with men of science all over the world.

Nidation of the fertilized ovum

M. C. SHELESNYAK

The study of generative processes in the mammal has commanded much attention during the past half-century, but although our understanding of the conception and development of the individual has become clearer, some serious gaps in our knowledge persist. The early stages of gestation—especially those associated with the implantation of the fertilized ovum as the blastocyst—remain vague. This article discusses certain special aspects of the general problem, but does not present a complete view of the progress of investigators exploring progestation. Emphasis is placed on the development of a concept of the mechanism of nidation.

Recently, three basic concepts and premises relating to the nidation (implantation) of the ovum were set out [7]:

1. In the present state of knowledge it is impossible to give any precise definitions of implantation, whether in spatial, temporal, or physiological terms. We do not know enough to say just when and exactly where implantation occurs, or what are the exact hormonal, vascular, biochemical, and nutritional requirements for its occurence.
2. There is a mutual relationship between the ovum and the endometrium, neither being solely responsible nor entirely active or passive.
3. The immediate reactions between blastocyst and endometrium can be achieved only within a 'proper' systemic environment which involves an interplay of hormones, of vascular state, and of nutritional, biochemical, and genetic factors.

The attack on the problem of the mechanism of nidation can be focused on various states during which it occurs: (*a*) normal pregnancy; (*b*) natural variants of pregnancy such as delayed implantation and ectopic pregnancy; and (*c*) specifically localized implantation (the elephant shrew *Elephantulus*, and fruit-bat *Pteropus gigantus*).

Here it may be of value to sketch briefly the history of the fertilized ovum in the mammal (for detailed presentation see [1, 2]). After fertilization, cleavage takes place. This is a special case of mitotic division. At the eight-cell stage, there is a special tendency towards centrally placed cells with a radial arrangement of spindles of division: this is the morula. In subsequent divisions the peripheral cells divide more actively. At this time, fluid collects in the intercellular spaces between the inner cell mass (the central cells) and the trophoblastic (peripheral) cells. The resulting small vesicle, consisting of a hollow ovoid of cells with an eccentrically placed inner cell mass, is the blastocyst. By the time the blastocyst stage has been reached, the egg has already traversed the uterine tube.

How the eggs reach the uterine tube after ovulation remains uncertain. Passage of the fertilized ovum through the uterine tube, which takes place while cleavage is in progress, is usually achieved in most mammals in three or four days. In the opossum it takes but one day; in the dog and cat about seven. Remarkably, the time is independent of the length of tube, its calibre, and the length of the gestation period.

In the uterine lumen the blastocyst remains free for a period that varies greatly from species to species. In the mouse and rat it is about 36–40 hours; in the guinea-pig, slightly longer; in the rabbit, 4 days. At the end of this free period the blastocyst becomes implanted. In the human, implantation takes place 6 days after ovulation; in the macaque monkey, 9 days; in the cat, about 13 days. In a number of mammals (at least eleven species of three very diverse orders of Eutheria) free uterine existence may be from several weeks to as long as ten months (European badger, *Meles meles*). So prolonged a period is called delayed implantation: deferred implantation is a preferred designation and is being more generally used.

In addition to this naturally occurring deferred nidation there are some species (rodents, certain insectivores, and marsupials) in which nidation is deferred (preferably described more precisely as delayed implantation) by physiological factors associated with lactation.

During the period of the free blastocyst, placement and spacing of the ova take place; also pre-implantation changes occur in the uterus under the control of the ovarian hormones dependent on the hypophysis. There is general agreement that progesterone (luteal hormone of the

ovary is required for maintaining the blastocyst and for preparing the endometrium.

The stage following nidation is placentation, or formation of the placenta. This is defined as an apposition, or fusion, of the foetal membrane to the uterine mucosa for physiological exchange [1, 5]. Our interest in this stage is mainly that the actual implantation of the blastocyst marks its onset, and that certain physiological processes which occur with the development of particular cellular components during pre-nidation play vital roles in placentation. Specifically, it is the decidua (the maternal component of the placenta, composed of decidual cells that begins its development before actual implantation. In some species decidual cells appear at the site of implantation before the egg implants. In others they may appear afterwards, but we believe that the initiating stimulus occurs earlier, and that it is in fact associated with blastocyst implantation.

Let us return to the specific problem of nidation. We have ascertained the general picture described above from anatomical studies of the progestational (using the word in the manner of French workers to designate the period of pregnancy prior to actual implantation) female genital tract. Histological changes in the endometrium are evident after ovulation. These changes, apparently in preparation for the reception of the fertilized egg, regress in the absence of fertilization. If fertilization is successful, the endometrium continues to make ready for the egg. Various substances accumulate in the uterine tissue. Polysaccharides, vitamins, and a multiplicity of enzymes have been revealed by recent histochemical and cytochemical techniques. Studies of the uterine vasculature give dramatic evidence of changes during progestation. These apparently permit increased generalized and localized delivery of blood. Pinpoint development of the vascular bed has been seen in association with sites of implantation. But the real or apparent mechanism of the location of sites has not been elucidated. Microscopic examination of the fertilized ova in the uterine tube permits us to follow gross growth. Inferences can be drawn from structure: thus, the trophoblast is credited with lytic activity, even with invasiveness, but biochemical evidence for these processes is lacking.

It is evident that morphological contributions have been fundamental for physiological and biochemical work. However, the derivation of mechanisms of the blastocyst-uterus interactions that lead to nidation cannot be gained from solely anatomical studies, although cytochemical and histochemical examinations, based on hypothetical mechanisms, can lead to corroborative information.

Physiological factors associated with normal pregnancy and with delayed implantation have been studied by many investigators, but in the main they have concerned themselves with little more than the hormonal requirements. Frankel's pioneer work on the necessity of the corpora lutea of the ovary for implantation has been amplified and extended by many investigators (see [3]). It is well established that a proper balance between the follicular hormones (oestrogens) and those of the corpora lutea (progestagens) is essential. The precise balance—a shifting one—is, however, yet to be established. We know also, primarily from studies on delayed implantation, that oestrogens are essential for implantation.

Precisely how the hormones establish the proper milieu for the blastocyst, and condition the endometrium so that the uterine lining can respond to the presence of the unimplanted blastocyst, has not been determined. Current work on enzyme-hormone relationships [16, 17] is helping us gain an understanding of the possible role which the oestrogens can play in initiating implantation. The recent finding that an oestrogen, as the initial activator, releases histamine [21, 23] gives oestrogens a part in the so-called histamine concept of nidation [22, 24].

Beyond the estimate of hormonal requirements, little is known regarding metabolic needs for implantation. Certain vitamins are apparently required, since their absence results in failure to implant. The ionic and energy requirements essential for implantation have yet to be defined, although the fact that the blastocyst metabolizes anaerobically has been reported. Calcium appears to play a role. Histochemical studies of the uterus of early pregnancy [8] and of placentae [26] are becoming more common. But this fragmentary collection of data has not led to a comprehensive picture of the environment of implantation. The elegant and meticulous work of C. Lutwak-Mann [12], by which she and her associates have gathered information about the rabbit-blastocyst weight, and various components (N, P, Na, K, Cl, reducing sugars, bicarbonate), metabolic turnover, and reactions to hormonal and toxic agents, continues to be developed and extended by the Cambridge group. Recently R. A. Popp [18] employed C^{14}-labelled compounds in conjunction with autoradiographic and chromatographic techniques on mouse blastocysts and uteri. These

biochemical researches will be of increasing value not only in collecting data, but in helping to postulate mechanisms and to test concepts already proposed.

Experimental techniques offer wide scope for studying natural nidation and delayed, deferred, and ectopic implantation; for exploiting various aspects of comparative embryology; and for studying artificial conditions associated with particular phases of ovum implantation.

Interference with implantation can be attempted by modifying the hormonal milieu: depleting by extirpation of endocrine glands, or supplying excess by gland-grafts or injection of hormone preparations. Another alternative is to alter the physiological status by other than endocrine means. Interference by altering the metabolic environment of the ovum-uterus complex, or by nutritional, biochemical, physiological, or mechanical means, has been attempted sporadically but not systematically or within a framework of hypothesis. Drug action in early pregnancy has received attention, primarily the study of toxic agents and antimetabolites [19, 26]; but analysis of factors of implantation by the use of these drugs has not been possible. The use of surgical techniques—blastocyst transplantation, reversal of uterine segments, crowding of blastocysts, denervation of the uterus, and alteration of blood supply—has given many data, but as yet no consistent pattern.

Several years ago the author turned his attention to the mechanism of nidation and designed experiments to exploit the phenomenon of 'deciduomata formation'. In 1907, L. Loeb [11] observed that injury of the guinea-pig uterus resulted in the growth of a discrete mass of endometrial cells. Since the cells were stromal cells transformed into decidual cells, the growth was called deciduoma.

During the decades that followed Loeb's original observation, a great deal of information about the deciduoma has accrued. Essential to our problem are these facts: (1) the deciduoma, artificially arrived at, mimics in structure and function the early maternal placental tissue; (2) hormonal requirements for successful decidualization are similar (if not identical) to hormonal requirements for successful ovum-implantation; (3) the uterus, sensitized by proper hormonal action, can be provoked by an initiating stimulus to develop a cell reaction.

It was clear that although the hormonal balance required for implantation was fairly well defined (so we considered), the means by which the blastocyst communicated its presence, and provoked the decidual response (if it did so), were unknown. After deciding to use the deciduoma and its induction as a model system for experiment, the first task was to establish the nature of the stimulus or decidua-inducing factor.

A great variety of stimuli provoke the formation of deciduoma in an animal with a properly sensitized uterus. Thus the search for a specific inducer started by seeking a specific inhibitor of a suspected inducer. Since the many methods used to provoke deciduoma formation all involved some degree of injury, the initial study was made on metabolites associated with injury and their specific inhibitors. The first system to be considered was that of histamine-antihistamine.

The fact that instilling traces of histamine solution into the hormonally sensitized uterus of the rat resulted in large deciduomata was not in itself useful evidence. However, when histamine antagonists were placed in the uterine lumen no decidual development occurred. It could be assumed from these data that histamine plays a role. This inhibition by antihistamines was effective, no matter what the means of stimulating the endometrium. The results of these preliminary experiments strongly suggested a metabolic role for histamine in inducing the decidual cell reaction, but they could not be considered proof. Antihistaminic drugs are notorious for having a variety of ancillary actions, and severe local tissue damage, or localized upset of required hormone balance, may have occurred. Since the antihistaminic inhibition of decidual cell development was basic to the attempted unravelling of the mechanism, it was necessary to establish the validity of specific histamine block as the basic cause of the failure of the decidual cells to develop [20].

Experiments were carried out using drugs with primary actions that were ancillary to the histamine-antagonism of antihistamines, such as local anaesthetics, vasopressors, and oxytocins. They failed to suppress the decidual reaction except where they mimicked a histamine inhibitor. Cytological observations of mitotic activity after the antihistamines, and comparison with corrosive tissue irritants, ruled out the possibility that tissue destruction was the basis for failure of the deciduoma to develop. Alteration of hormonal balance did not reverse or prevent the antihistamine inhibition of decidualization [20].

If histamine was, in fact, a metabolite capable of inducing the decidual cell reaction, it should have been possible to provoke the decidualizing

process in the hormonally sensitized uterus by making histamine available to the endometrium by systemic means, without traumatizing the tissue. This was successfully achieved by P. F. Kraicer and the author [10], who induced decidual reactions in pseudopregnant rats—pseudopregnancy offers a blastocyst-free uterus, which is none the less hormone-sensitized—with massive intravascular and intraperitoneal injection of histamine or of histamine-releasing substances. There was no physical contact with the uterus.

This technique not only gave added evidence of the role of histamine in the induction of deciduoma, but also afforded a critical trauma-free method for study of the decidual response in blastocyst-free pseudopregnant, and blastocyst-bearing progestational, uteri.

Armed with the information assigning histamine a role in inducing 'artificial' decidualization, it was possible to turn attention to the decidual process associated with normal implantation of the blastocyst. Initial experiments showed that instilling very small amounts of antihistaminic solutions into the lumen of the progestational uterus completely blocked decidualization and concomitant implantation, so that pregnancy was terminated. There was now enough indirect and collateral evidence to seek out histamine-related conditions in the progestational uterus.

The first approach was to measure the mast-cell [29] population. It is well established that, in the rat, mast cells contain reservoirs of histamine, and that, moreover, the disruption and disappearance of tissue mast cells which follow the administration of histamine releasers are evidence of an 'unbinding' of histamine. Complete longitudinal sections of progestational uteri were stained for mast cells, and the number in the endometrium counted. Measurements made at short time-intervals during the pre-implantation period revealed an almost complete depletion of mast cells during the 24–36 hours before physical embedding of the blastocyst was observed [24].

This indirect, morphologically based, evidence that histamine was released and metabolized in the uterus during the period before ovum-implantation warranted a direct study of uterine tissue for histamine at parallel times. Progravid uteri of pregnant rats were treated with trichloracetic acid to extract tissue histamine, and the ether-washed extract was assayed biologically by use of the atropinized guinea-pig gut. The data were entirely consistent with the mast-cell population picture [22] (figure 1).

On the basis of existing evidence it could be concluded that histamine induced the decidual cell reaction. That the act of induction of decidualization led directly to ovum implantation was as yet an unproven assumption. Several possible tests could be applied. From studies of delayed implantation, where the delay was produced physiologically during lactation, or was experimentally induced [13], it is known that small doses of an oestrogen induce decidualization and blastocyst invasion of the endometrium. If the action of the oestrogen is dependent on the histamine mechanism, two conditions should be satisfied: (1) oestrogens, like histamine, should induce deciduoma; and (2) oestrogens should release histamine. In experiments carried out in rats, administration of a single intramuscular injection of an oestrogen to a rat with a properly prepared uterus provoked deciduoma [9]. The first condition was thereby satisfied. Recent studies of histamine content of, and concentration in, rat uteri after systemic injection of an oestrogen, showed that the oestrogens cause a release of uterine histamine in the rat [21, 25]. This satisfies the second condition.

Although the consistency of the behaviour of oestrogens in the pattern of histamine-induction of decidualization is proved, the question remained as to whether the induction of decidual cell reaction in a uterus containing blastocysts will lead *per se* to blastocyst implantation. This query could be answered by using experimentally established conditions of delayed implantation in the rat and testing whether the induction of decidualization by histamine liberators does in fact provoke the 'resting' blastocyst to implant in the uterus.

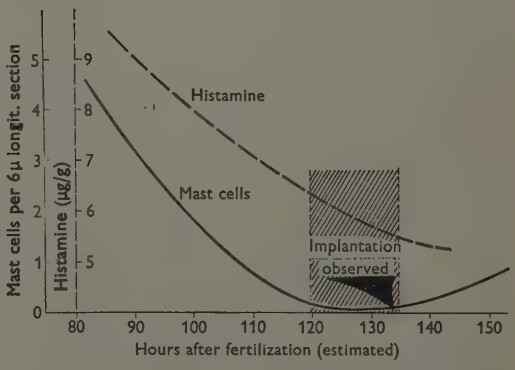


FIGURE 1 – *Variation of the number of endometrial mast cells, and of the concentration of histamine in the uterus, during early pregnancy.*

By using the techniques of French workers [3, 4, 13], it was possible to prepare rats with fertilized eggs—blastocysts—retained in the uterus (delayed implantation). In normal pregnancy the blastocyst will become implanted about 5–6 days after mating. In the experimental animals the blastocyst can remain intact and quiescent in the uterus for several weeks. The administration of small amounts of oestrogen will induce implantation. We attempted to provoke implantation in rats during the period of experimentally delayed implantation by inducing the decidual process with a systemic histamine release [9]. The released histamine did provoke decidualization, but the blastocysts were not induced to implant, in spite of a massive engulfment by decidual cells.

Here, then, we were confronted with the fact that though decidualization is an essential component of the overall process of nidation, the induction of an active decidualization process in a uterus containing blastocysts (presumably ready for implantation) failed to cause the implantation or invasion. Clearly the mechanism involved at least two parts, and, while the histamine-releaser failed to activate the quiescent blastocyst, the oestrogen succeeded. Since oestrogens release histamine, and so are capable of inducing decidualization, one must presume an auxiliary action of the oestrogen on the blastocyst-endometrium complex, either directly or indirectly, and must suppose that this action is synchronized with the induction of decidualization.

The synchronization was examined by injecting histamine-releaser (to provoke decidualization) at precise times during pseudopregnancy and during progestation. It was possible to show that the uterine sensitivity to decidualization occurred for a very limited time at the same stages in both progestation and pseudopregnancy. This coincidence in time has also been demonstrated in studies employing the transfer of ova of various ages (3–5 days) to uteri at varying stages of pseudopregnancy [5, 14, 17].

These data have permitted the writer and his co-workers to postulate a mechanism of ovum-implantation which lends itself to test and which provides a basis for further investigations focused on specific biochemical aspects of the blastocyst-endometrial relations. The critical problem of communication may be explored.

The postulated mechanism is this. Following successful fertilization of the ova, the corpora lutea—which started developing at the site of the ovarian follicle directly after ovulation—persist and continue active secretion of progestin. The physiological status of the genital tract is under its control, and progestin dominancy is essential for the normal cleavage of ova, and for their migration to the sites where the ova (now blastocysts) will implant. During the period of progestin predominance, there occurs a brief episode which we describe as an oestrogen surge. This systemic or local oestrogen has its primary impact on the blastocyst-endometrial complex, initiates active decidualization of the uterus, and at the same time activates the blastocyst for implantation into the endometrium at that time when the latter is receptive (figure 2). The source of the oestrogen surge, we surmise, depends on the species, and is related to the nature of its oestrous cycle. In the rat, which has a 4–5-day cycle with spontaneous ovulation, we have reason to suspect that the ovary is the primary source of the oestrogen surge, and that the surge occurs at that time when a pre-oestrous rise would occur normally, provided the rat had not become pregnant. The progestin of pregnancy has apparently damped down, but not completely suppressed, the 4–5 day oestrous cycle. It is also considered possible that in this species the blastocyst also carries some oestrogen. We postulate also a substance (possibly associated with the zona pellucida of the blastocyst) which augments the histamine release, either indirectly, through the oestrogen surge, or directly. In species where ovulation occurs only after coitus, such as the rabbit and the cat, we surmise that the oestrogen surge originates directly from the blastocyst or its membranes.

The oestrogen surge is followed by histamine liberation, and the released histamine induces the

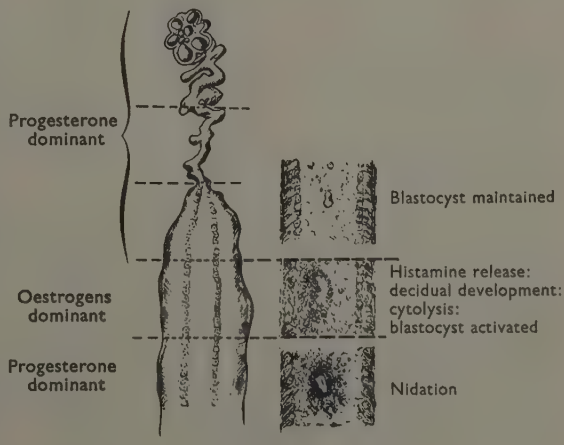


FIGURE 2

stromal cells of the endometrium to transform into decidual cells. As the process of decidualization begins, tissue lysis—an activity measured in terms of release of protein by the endometrium—can be detected before any morphological changes can be seen. This activity produces a medium more receptive to the invasion of the blastocyst. Thus nidation, in simplest terms, consists of a two-phase process: nidus formation (the preparation of the endometrium, basically decidualization), and nidus invasion (by the activated blastocyst), triggered by an intrinsic oestrogen surge. Both blastocyst and endometrium play roles, and these roles are monitored by ovarian hormones and associated hormones and metabolites. Figure 3 is a schematic presentation of this concept.

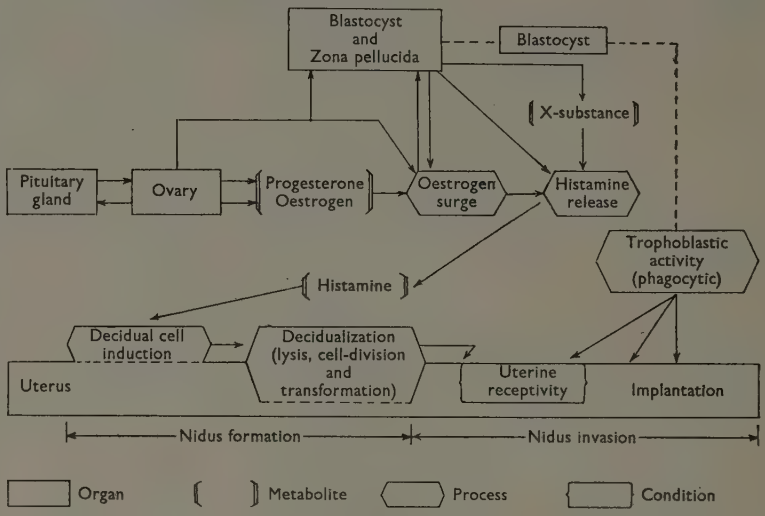


FIGURE 3

Much is lacking for a clear understanding, particularly about the means of communication, but the hypothesis affords opportunity for a more systematic analysis of the mechanism of nidation.

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The origin and nature of the Moon

H. C. UREY

The photographs of the far side of the Moon that were sent back from the Russian satellite have stimulated interest in the origin and nature of the Moon. The author of this article discusses the present theories, and suggests inferences concerning lunar structure that may be drawn from the photographs. Other artificial satellites of the Moon will be launched, and the contributions to our knowledge that may be made by these are also considered.

THE ORIGIN OF THE CRATERS

In 1893 G. K. Gilbert, who appears to be the only student of the Moon's surface in the course of the last century who had any sound knowledge of physical geology, published a remarkable paper on 'The Moon's Face' [1]. He reviewed many features of the lunar craters and concluded that they were due to great collisions of meteorite-like objects with the Moon's surface. In recent times, R. B. Baldwin in his book 'The Face of the Moon' [2] reviewed the evidence concerning the origin of the craters, and since then it has not been necessary to reconsider the problem. Baldwin gives many references to the older literature and considers in detail the alternative hypotheses of a volcanic or a collisional origin. The volcanic theory of crater origin was advanced before modern scientists realized that meteorites fell on the Earth, and it required nearly a century of discussion before astronomers agreed that most craters resulted from collisions.

Gilbert concluded that lunar structures are not similar to those of the Earth; that the pattern of overlap is that to be expected for chance collisions; and that the Moon has no structures similar to terrestrial volcanoes, if account is taken of the absence of erosion there. He recognised, however, that there are small craters which cannot be due to collisions and hence must be of some volcanic type, even though their shape is not that of terrestrial volcanoes.

Many questions relating to the Moon are decisively answered by consideration of its overall shape and the nature of one of its principal features, the great Imbrian collision. There are many other subsidiary lines of evidence, but we can best begin our account by discussing these two important aspects.

THE SHAPE OF THE MOON

Measurements of the elevations of the lunar surface have been made, but the methods of observation are difficult and not very precise: reliable evidence that it has an irregular shape comes from its dynamical motions. Textbooks on celestial mechanics give a formula for computing the difference in the moment of inertia about the polar axis (C) and that about the axis pointing towards the Earth (A). From observed data $(C-A)/C=0{\cdot}000\,629$, whereas the theoretical value deduced from consideration of the Moon's shape under its own gravitational field, the Earth's gravitational field, and the centrifugal forces of rotation, is only $0{\cdot}000\,037\,5$. The ratio of these numbers is $16{\cdot}7:1$. If the Moon has a uniform density, the observations require that the radius towards and away from the Earth should be larger than that towards the poles by about 1 km, whereas theory requires a difference of only about 60 m. This irregular shape must result in a difference in stress at the centre of about 20 atm, and this requires considerable strength of the material at the Moon's deep interior if the density is uniform throughout.

If the density of the Moon is not uniform, but varies according to latitude and longitude by a small amount, the irregular shape can be explained by a theory recently advanced. This requires the density near the poles to be larger than that about the axis pointing toward the Earth. In this case, the strength of the deep interior need not be very great, and the centre could be at a high temperature. But this variation of density with latitude and longitude could hardly have been preserved if the Moon had ever been generally molten.

But could the Moon have been molten even if it has a uniform density? So large an object can cool only very slowly, even in a time as long as the Moon has existed, which we believe is about 4·5 aeons (an aeon being defined as 10^9 years). This age is assumed to be the same as that of the

meteorites. Calculations of the loss of heat show that the centre of the Moon would lose little heat even in this length of time. If the Moon were ever molten, its centre would still be very close to its melting point and hence would not have the required strength to support the irregular shape.

The presence of the radioactive elements potassium, uranium, and thorium would increase the internal temperature, the effect depending on the amounts of these elements and their distribution. We know nothing about the distribution of these elements in the Moon and have difficulty in estimating their distribution in the Earth. However, if the heat being lost from the Earth is assumed to be entirely due to radioactivity, we find that the total amounts of these elements in the Earth may be very similar to that of the meteorites and that possibly less than one-half of them are in the crust. In the Moon, a similar distribution would keep the temperature in the deep interior above the melting point if it was originally molten. In fact, the entire interior out to about 0·8 of its radius would be at the melting point if the initial temperature had been that at which silicates melt. Such a conclusion is, however, inconsistent with the shape of the Moon, and it must therefore have been formed at a low temperature.

We may ask whether the interior of the Moon would be molten at the present time, due to radioactive heating, even if it had been formed at a low temperature. There are uncertainties in the answer to this question. We do not know the concentrations of the radioactive elements in the Moon. Possibly the concentrations in meteorites give the best estimate, though it is not a certain one. Calculations indicate that any metallic iron-nickel in the Moon's deep interior would be molten, but we have in fact no evidence for the existence of metallic iron within the Moon. The material of the Moon is not a pure substance, and hence would not melt at one temperature: calculations indicate that partial melting of the silicates might occur. All these considerations can be reconciled only with the hypothesis that there is a variation in density with latitude and longitude. This explanation is consistent with a partially molten interior today, but inconsistent with a generally molten condition in the distant past.

Gilbert said 'During the whole period of growth the body of the Moon was cold.' This statement was made before the discovery of radioactivity. It has required much argument to come to the same conclusion again, and the problem is a critical one [3].

In 1862 Kelvin [4] wrote a paper dealing with the solidification of the Earth from a completely molten state. At that time no other source of heat for volcanic processes was known other than residual primitive heat, and he therefore assumed a high-temperature origin for the Earth. This belief has found its way into the textbooks, and it is now generally assumed that all objects in the solar system were once at very high temperatures. The discovery of radioactivity at the turn of the century made Kelvin's assumption unnecessary, but no reconsideration of the whole problem was made at that time. Possibly planets and satellites were formed at high temperatures, but we may now well ask what evidence exists for or against the hypothesis.

THE IMBRIAN COLLISION

A very great collision occurred in Mare Imbrium at some time in the past: the evidence for this was discovered and described by Gilbert. The collision directly modified a large fraction of the visible hemisphere of the Moon. The region of Mare Imbrium is shown in figure 1, which is a composite picture of the Moon: it is the large grey oval area at the lower right of the picture. It is also shown in figure 2, which was made by projecting pictures on to a white sphere and then photographing the mare from a point directly above it. The lines indicate the positions of the Bay of the Rainbow or Sinus Iridum, a circular area before it, and the entire mare. Radiating from the circular area are many ridges, particularly towards the south-west: there are also many grooves in the surface, as can be seen on good photographs. Gilbert noted all these features, except for what seems to be an obvious relationship of Sinus Iridum to the rest of the pattern. The most reasonable explanation of all these details is that they are related and were produced by one event. The whole pattern is unsymmetrical, and hence one concludes that the colliding object arrived from the north-east at a substantial angle from the vertical, ploughed a deep hole in the Moon, and spread out some of its own substance and some of the lunar substance in a wide fan-shaped area and to great distances. The object probably came in through what is now Sinus Iridum and either produced this bay or destroyed one wall of a walled plain. Alternatively, a second collision of substantial size occurred in precisely the same region after the formation of Mare

Imbrium. Two large collisions at the same spot seem most improbable, and hence it is most likely that one great collision produced the entire pattern.

A few of the ridges and grooves extend through the Jura mountains to the north-east of Sinus Iridum. The principal pattern spreads out in a semicircular fan from the edge of the Carpathian mountains on the east, to Plato to the west of Sinus Iridum. Mountainous ridges which point towards the circular collision area are found in and beyond the centre of the Moon's disk. High-density objects ploughed through the walls of Ptolemaeus and Alphonsus and produced great grooves in the surface: these objects can reasonably be assumed to have been metallic iron-nickel. In Oceanus Procellarum there are many short ridges orientated in the general direction of the collision area. It is not clear whether some of the individual ridges and grooves belong to the system or not, but the overall pattern is entirely convincing.

The circumsurface velocity of the Moon is 1·7 km per second, and since the objects fell at a point some 1000–1500 km from the point of collision, their velocity must have been of about this value. The broad pattern indicates that the object was moving at a velocity lower than that of sound in the material of which it is composed—in this case, some 5–7 km per second. An object moving with relatively high velocity—say some 30 km per second—would bury itself before the rear side received a signal that contact was made and would explode to give a symmetrical pattern. But if we assume a velocity less than that of sound—say some 2·4 km per second, which is the escape velocity of the Moon—the unsymmetrical pattern can be understood. Vertical deceleration of the top of the object would occur, and this part could move off horizontally at some 1·7 km per second: probably some would spray out sideways to produce a fan-like distribution. The object may have been a satellite of the Earth–Moon system or one moving in an orbit similar to that of the Earth.

Whatever its nature, the object most probably produced Sinus Iridum, and hence passed between the two promontories of Laplace and Heraclides: the distance between these is about 230 km, and this is therefore the maximum diameter of the planetesimal. Gilbert suggested 100 miles for its diameter: other suggestions have been made, but on less direct grounds. Using 200 km as a likely value, calculations of the object's kinetic energy can be made. Assuming a density of 3·5 g/cm^3 and 2·38 km per second (the velocity of escape from the Moon) for the velocity, the kinetic energy is $4{\cdot}15 \times 10^{32}$ ergs. This is equivalent to $4{\cdot}6 \times 10^{11}$ atomic bombs, equivalent to one for each 1100 square metres of the Earth's surface. The largest earthquakes are estimated to expend about 10^{24} ergs, and thus the Imbrian collision dissipated an energy more than 10^8 times as great.

A collision of this magnitude is completely beyond any observations which we have made, and deductions made from any scaling-up of terrestrial observations would be most doubtful. It is better to take this collision as an observed fact and to try to learn something about large collisions from it. The object ploughed in through Sinus Iridum; flattened out in the collision area; and raised a great bulging wave in the Moon's surface in all directions, but particularly in the forward direction, that is, towards the centre of the Moon's visible disk. The affected area was badly broken up, perhaps even to the consistency of fine sand, and after the collision, part of the material subsided again, producing the shelf area between the inner and outer rings shown in figure 2.

It is possible that part of the lunar surface was lifted and then dropped as big blocks, forming the Straight Range, Piton, Pico, Spitzbergen, and the other mountainous mass indicated by the arrows in figure 2. The Alps, Caucasus, Apennines, and Carpathian mountains may also be formed of this kind of material; the first two, in particular, look like fragments of this kind. It is, of course, possible that those mountains were part of the planetesimal. The Haemus mountains must consist of fragments of the colliding body. The long grooves must have been produced by high-velocity and high-density objects ploughing through the surface. Because of their high density, these materials must have been metallic iron-nickel, and they must have been part of the colliding object in spite of their great distance from the collision area. Such iron-nickel objects could hardly have formed a core of the colliding planetesimal: probably they were distributed as isolated objects, mixed with silicates, throughout the mass. They are reminiscent of the iron meteorites, and probably the planetesimal was similar to objects which produced the iron meteorites.

Other circular maria are Crisium, Nectaris, Humorum, and Serenitatis. A prominent scarp, the Altai Mountains, to the south and east of Mare Nectaris, is similar to the scarp around Mare Imbrium. The area between the mountains and the smooth mare is covered with great craters: it

seems to be analogous to the shelf-area in Imbrium. The Rheita and Borda valleys and other grooves radiate from this mare, and may have been produced by high-velocity missiles. Baldwin believes there are radiating grooves around other maria. Partially covered craters are present in Mare Serenitatis. Probably all these circular maria were produced by great colliding objects which approached at differing angles to the surface. The highly unsymmetrical character of Mare Imbrium shows that in this case the planetesimal approached at a low angle. The other collision maria were probably produced by objects falling more nearly vertically and hence did not produce such a wide-angle fan of ridges and grooves.

THE TIME OF OCCURRENCE OF THESE EVENTS

Some of the craters on the Moon must have been produced by meteorites, though we have little evidence for really large meteorites hitting the Earth. Some of the large lunar craters were formed before the Imbrian collision and others afterwards. Ptolemaeus (figure 4) has walls that have been scarred by missiles from the Imbrian collision and are very low, as though they had been partly shaken down by this energetic event. Mountainous masses have fallen on other craters, as for example in Julius Caesar. Other craters, such as Aristillus and Autolycus and some lying within Mare Imbrium and on the southern edge of Aristophanes, are more recent. Whether Plato and Archimedes could have survived this great collision is doubtful. Similar relationships are evident in other maria. Theophilus and other craters in the neighbourhood are more recent than Mare Nectaris. All these collisions were part of a single series of events; some craters were formed, then a mare, then still more craters, and so on. These craters and maria cover the entire visible surface of the Moon so densely that the whole of it has been broken up. One would expect that during the time of this bombardment the Earth would have been bombarded even more intensely than the Moon, because of the greater energy of the collisions with its surface and because its gravitational field gives the Earth a larger collision cross-section. Such a bombardment would have destroyed all the terrestrial sedimentary rocks and should have left great scars on the continental shields. The oldest terrestrial rocks have been reliably dated at about 3 aeons, while the meteorites are about 4·5 aeons or slightly older. The intense bombardment which occurred on the Moon must have happened more than 3 aeons ago, and may well be a record of the final stage of the formation of the Moon, the Earth, and indeed of the Solar System.

COMPOSITION OF THE MARIA

It has been commonly supposed that the maria consist of solidified lava. This is a natural assumption, since the maria are level, as we would expect fluids to be in the Moon's gravitational field. It has been assumed that the lava flowed from the Moon's interior, but a well-known line of reasoning shows that this is doubtful. Terrestrial lavas have temperatures not over 1200° C, which is a reasonable estimate of the melting points of basaltic liquid in equilibrium with the silicates beneath the Earth's surface. This liquid cannot be much above its melting point, because if it was, it would melt the containing walls and hence cool until equilibrium was again reached. If such a liquid flows on to a cold surface the lava begins to freeze, and it is difficult to understand how it could flow very far without producing fairly high terminal walls. It should be noted that proponents of the ideas that the maria are of laval origin have postulated that the liquid flowed very great distances. The Oregon and Deccan plateau lava-flows each cover about 500 000 km^2, which is smaller than Mare Imbrium. If the lunar features are indeed lava-flows, they are comparable to, or even larger than, those on Earth. It seems probable that some terminal walls would be preserved if the great smooth areas were in fact of this type [5]. Also, had the Moon's surface been underlain by these vast quantities of high-temperature lavas, the great mountains would surely have gradually sunk into the Moon's surface. On the basis of such arguments Gilbert concluded that the Moon was cold when its maria were formed, and we can conclude that it has been cold in its outer parts ever since. There is no evidence that the mountains have settled into the Moon's surface, and we can deduce from this that the lavas (if they are lavas) did not come from the Moon's interior.

Gilbert believed that the melting was caused by the collision energy of the objects arriving on the Moon. The collision energy per gram of an object arriving with the escape velocity of the Moon (2·38 km per second) is 2800 joules, and about 2000 joules per gram are required to heat silicates to the melting point and then to melt them. Some energy would be dissipated as vibration, but some melting might occur. If the

FIGURE 1 – *The Moon. The mountains formed by the spray of material from the Imbrian collision can be seen in this picture.* (*Leck Observatory photograph.*)

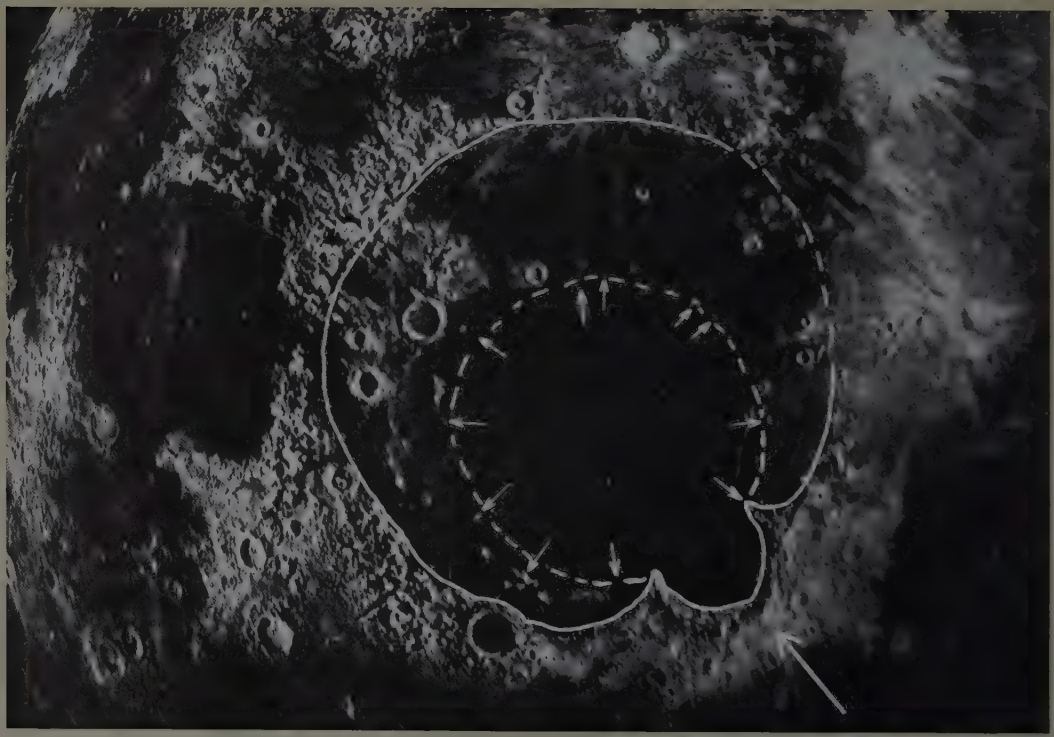
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FIGURE 2 (*left, top*) – *Mare Imbrium, with foreshortening eliminated. Mare Imbrium is outlined by the solid curve. Sinus Iridum is the bay at the lower right. The arrows indicate mountainous masses just outside the collision area. Three of these arrows point to masses not visible in this picture but easily seen on others.* (*University of Chicago photograph.*)

FIGURE 3 (*left, bottom*) – *Maria Imbrium and Serenitatis, showing the orientated mountain structures radiating from the collision area in Mare Imbrium. These structures show that the Imbrian collision occurred after the collision that produced Mare Serenitatis.* (*Mount Wilson and Mount Palomar photograph.*)

FIGURE 4 (*above*) – *Ptolemaeus, showing the many small craters. The surface is not smooth, and the smooth grey material covers some craters which antedated the formation of the material which covers the floor. The walls have been scarred by projectiles which arrived from the north-north-east, and which were part of the debris from the Imbrian collision.*

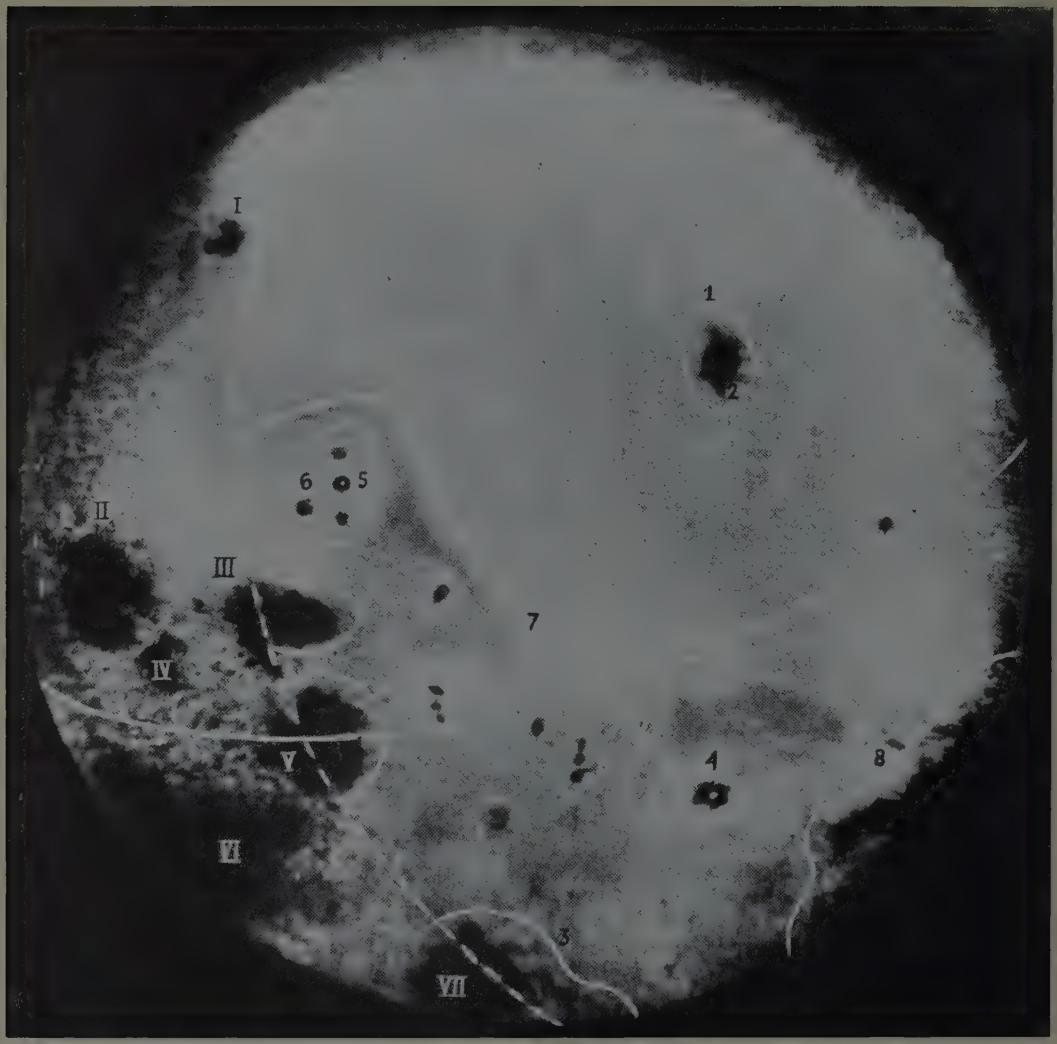

FIGURE 5 – *The other side of the Moon as seen from Lunik-3. The dotted line shows the edge of the visible part of the Moon. All features to the right of that dotted line are invisible from the Earth. Features marked on the visible section are (I) Humboldt Sea, (II) Sea of Crisis, (III) Regional Sea, (IV) Sea of Waves, (V) Smith Sea, (VI) Sea of Fertility, (VII) South Sea. Newly seen features:* (1) *Moscow Sea, about* 300 *km across,* (2) *Bay of Astronauts,* (3) *the hitherto unknown section of the South Sea,* (4) *Tsiolkovsky crater with its central peak,* (5) *Lomonosov Crater with its central peak,* (6) *Joliot-Curie Crater,* (7) *Soviet Mountain range,* (8) *Sea of Dreams.*

velocities were very high, volatilization would occur and something like a great explosion would take place. This is inconsistent with the unsymmetrical pattern of ridges as mentioned before, but nevertheless a velocity somewhat higher than the escape velocity is possible.

T. Gold [6] has suggested that the maria consist of deep layers of dust produced by the eroding effects of sunlight and particle radiation from the Sun. He believes that such particles would move over the surface because of charges on them. A hopping motion is assumed to have moved them over great distances in the past and to move them in the same way at the present time. He points out that the many craters, large and small, in the southern regions have smooth, grey material within and between them, and that neither lava from the interior nor molten material resulting from collisions appears to be a reasonable explanation. Such reasoning prompts two questions. Do we think that liquid from the interior seeped up in all these places? If there were ever large quantities of liquid beneath the lunar surface, would not the more dense solid phase sink steadily down into the liquid until it was completely submerged?

However, the hypothesis that erosion was the main origin of the dust is unreasonable, for if this were so, all parts of the Moon surface should have the same appearance, or this should at least be true of all equatorial regions. It is not true of the hemisphere that faces the Earth and, as we have lately learned, emphatically not true of the back hemisphere. These facts make Gold's general hypothesis untenable, although some erosion of this type may be present and may modify the effects of other more dominant processes.

If spread over the Moon's surface, the Imbrian planetesimal, supposedly some 200 km in diameter, would make a layer 110 m deep, and if it contained one per cent of water by weight this would be enough to cover the Moon to a depth of 3·9 m. This is a larger percentage of water than is contained in the meteorites or on and within the Earth. Other planetesimals which produced the other circular maria could have supplied water also. Such planetesimals would be moderately explosive if they contained water or other volatile substances. These possibilities suggest that in addition to the materials which have already been discussed above as coming from these planetesimals, there may have been a vast dust cloud which spread finely divided materials over the Moon's surface. This material, of course, would fall on the mountain slopes, but it does not appear to be there now. Did temporary rains wash it off? Or did the violent moonquakes cause it to slide into the valleys, as 'fluidized' mixtures of gases and solids used in industrial chemical processes do? Gilbert remarks that many lunar features look as though they had been covered by a pasty material, and thought it was partially solidified lava. Perhaps he was right, and it is neither dust nor rubble.

To resolve the problem we should look for objective evidence. Anyone looking at the Moon through even a small telescope is immediately impressed with the smooth character of the maria, but I am convinced that merely looking in this way will never provide very detailed evidence. If only we could see the crystalline structure of the lunar rocks! Lava, especially basaltic lava, is a dense liquid of considerable fluidity which might push over and distort crater walls in its path. I can see no evidence for this having happened in Mare Nubium or Oceanus Procellarum. I have for the most part studied only pictures, but a little observing convinces me that gross features of this kind are not likely to be missed when good photographs are available. But lava must flow downhill, whereas dust can fall anywhere. The colouring in some photographs indicates a smooth hill over the eastern wall of Prinz and over the craters Hippalus and Weiss: other examples are less clear. On the other hand, the crater Wargentin is full to the brim with what appears to have been a liquid. Gilbert noted that Julius Caesar is filled as high as a breach in the southern wall and that Posidonius is filled to the height of a similar breach, which suggests that the contents of both were once fluid.

Mare Tranquillitatis has a very irregular shape and is very black. One would expect that the settling of dust would give the Moon a uniform colour or that colour variations would be gradual. The boundary between the black of Mare Tranquillitatis and the grey of Mare Serenitatis in the western part of the latter is very sharp, and there appears to be no difference in level. In fact, Mare Tranquillitatis does look like a lava flow. Some of its craters—for example, Ross—seem to be distorted. The fragments of craters in the western part of the mare look as though they may have been pushed about.

Possibly both lava and dust were produced by the collisions, sometimes one and sometimes the other. Possibly a planetesimal fell vertically and produced Mare Serenitatis and a pool of very dark lava which flowed into Mare Tranquillitatis.

It had only a small content of volatile substances, and its material was distributed to only a limited extent over the Moon. Then the Imbrian planetesimal, containing an appreciable concentration of volatile substances, fell and distributed grey dust widely. Possibly it was some other series of events. Gold's dust hypothesis has at least stimulated some serious thinking about the long-accepted lava hypothesis.

DURATION OF THE BOMBARDMENT

It is notable that there are no large craters of later date than the maria in the collision maria: in the case of Mare Imbrium this applies only to the collision area within the broken circle of figure 2. Theophilus and Piccolomini, near the shore of Mare Nectaris, and another on the Altai mountains at the south are certainly post-mare, as are other craters near the shores of other maria. These circular collision maria must have been fluid, that is, either true liquid or dust 'fluidized' by gas at the time the bombardment ended. A similar argument holds for the absence of mountainous masses in Mare Serenitatis. Had this mare been formed after Mare Imbrium, one would expect to find the scars of this collision scattered over the radiating ridges of the Haemus mountains, to the south of the mare, which were obviously produced by the Imbrian collision. Further, the mountains in the region between the two maria are very well preserved, and have a radiating pattern showing that they are part of the Imbrian system (figure 3). For these reasons I conclude that Mare Serenitatis is older than Mare Imbrium. But in this case the absence of a mountainous mass, similar to the Haemus mountains, on Mare Serenitatis, shows that this mare was fluid at the time of the Imbrian collision. Lava would very quickly solidify to form a very rigid mass. Dust or sandy material can become rigid also, though it might not for some time have sufficient strength to support a massive mountain. The small craters in the maria are due to collisions that took place during the 4·5 aeons that have elapsed since the occurrence of the very great processes which produced the maria.

These arguments indicate that the major surface features of the Moon were fashioned in a very short time. Calculations based on the rate of cooling of solidified rocky materials show that only some tens of thousands of years are required for such pools of lava to cool: if some low-density froth or pumice floated on the surface the time could be considerably longer. (In this connection it must be remembered that the solidification of most liquids does not follow the same course of that of water, whose solid phase floats on the liquid: if a magic wand were to make water behave like all other common substances, the ice of the Arctic ocean would sink and the ocean would freeze solid from bottom to top.) The consolidation of dust would require still longer. We have little experience on which to base a guess, but possibly much less than a million years would suffice—a surprisingly short time. It is possible that the objects which collided with the Moon to produce its gross features were satellites of the Earth–Moon system. During some thousands of years, such objects would collide with the Earth or Moon, but objects travelling in orbits about the Sun, when in the neighbourhood of the Earth would collide with the latter within some tens of millions of years, according to calculations of E. J. Öpik [7]. Objects moving in orbits about the Earth should arrive at its surface with about the escape velocity of the Moon.

The stone meteorites are immensely complicated structures, consisting of agglomerated sandy materials whose crystalline minerals were certainly formed in a body different from those in which they are now found. Some collisional process such as occurred on the Moon may have produced the highly fragmented crystals that we find in these meteorites. Using three radioactive dating methods—based respectively on the rate of change of uranium to lead, of rubidium-87 to strontium-87, and of potassium-40 to argon-40—we find that the time that has elapsed since they were last heated to high temperatures is about 4·5 aeons. It is probable that the Moon, the Earth, and in fact the entire Solar System were formed at that time. Any theory of these events must explain the curious physical structures and chemical composition of these meteorites.

It is most curious that the cosmic-ray ages of stone meteorites are only some tens of millions of years, and no certain explanation of this observation is available. However, objects moving in the neighbourhood of the Earth's orbit would be expected to exist in free space for about this length of time before they collide with the Earth. Thus for most of geologic time they must have been covered with a layer of screening material, about a metre in thickness, which prevented the cosmic rays from producing special varieties of atoms, in this case the inert gases helium-3, neon-21, and argon-38. Then they were broken out of this environment as objects so small that cosmic rays

could penetrate them. This requirement demands that their diameters should be in the range 30–100 cm. Thereafter they travelled in interplanetary space for some tens of millions of years. Could they have been blasted off the Moon by the heads of comets? This is a possibility, and if it were so we would know of what some parts of the lunar surface are composed. However, only the transport of some pieces of the Moon's surface to the Earth can decide whether this supposition is correct.

We may conclude that the Moon's surface was fashioned mostly by great collisions with its surface some 4·5 aeons ago during a relatively short period of time—probably less than a million years. Since then it has been bombarded by lesser objects which have produced mostly smaller craters.

THE OTHER HEMISPHERE OF THE MOON

The great space vehicles of the Soviet Union have given us a glimpse of the previously unknown hemisphere of the Moon: this feat must be rated as the greatest in exploration since the discovery of America by Christopher Columbus. This first observation (figure 5) gives us only a vague idea of what the 'other side' is like and what we may learn from it. It has fewer maria than the visible hemisphere; this is unexpected but not surprising, because the Imbrian collision has supplied so many grey smooth areas to this side, that is to say its own area and probably much of Oceanus Procellarum, Mare Nubium, and other neighbouring areas. If by chance the planetesimal had fallen on the other side of the Moon the general appearance of the two hemispheres would have been largely reversed. Certainly removing Maria Imbrium and Serenitatis would produce this general effect. The number of craters on this side is so great that it would not be possible by any stretch of the imagination to suppose that the great 'land' areas of the other side would not be covered by similar craters. Though we might suppose that an equivalent of Mare Imbrium would be found on the far hemisphere, it would not be surprising if this did not occur.

On the basis of pictures so far made available to the West by the scientists of the Soviet Union we are not able to draw any very important conclusions about the structure and history of the Moon. The maria on the limb of the Moon extend to the other side, and Mare Moskva (Moscow Sea) occupies the approximate middle of the further hemisphere. Again there is an indication that it has a neighbouring smooth area which might be due to a flow of lava from the area of a collision or to the fall of dust and rubble produced by such a collision. It is quite evident that general erosional effects from sunlight and particle radiation from the Sun have not been very effective in shaping the Moon's features.

OTHER FEATURES OF THE LUNAR SURFACE

Over the years, many features of the lunar surface have been recorded in considerable detail by many observers, and some new details have been added by recent observations. The rays from some of the craters cross all other features, both the mountainous and depressed areas. They are due undoubtedly to particles thrown out over the surface as a result of the explosive collisions which produced the craters. In the high vacuum existing on the Moon's surface, each dust particle would travel in an elliptical path, and if the particles from a crater moved in similar but not identical paths they would fall in a pattern which might or might not be that of the observed rays. If such particles were moving with a velocity of 1·7 km per second or greater, and if the initial direction were parallel to the lunar surface, they could travel completely round the Moon and arrive back at their starting point in somewhat more than 108 minutes. The Moon in this time would have rotated to the west, and the particles would therefore miss the original crater and fall on its eastern rim, if the orbit were in a north-south direction. Two rays of Tycho do just this. It has recently been suggested that, if particles were thrown vertically to a great height and fell back to the surface in about the same length of time, the same displacement would be observed. The rays of Copernicus are very irregular, and various explanations have been given of this. Possibly a slight atmosphere was present at the time when they were formed. Many well-formed craters have no rays: possibly they are older and the rays have become obliterated by the fall of micrometeorites, or perhaps, again, the Moon had a temporary atmosphere when they were formed. Proclus has rays extending in all directions except towards the south-east: Fielder has reported the presence of a ridge there which may have intercepted the particles. If this is the correct interpretation, we can conclude that the collision sprayed out this material in a horizontal direction. Chemical effects such as the escape of gases from below the surface may also have been important in producing this unsymmetrical ray pattern.

N. Kozyrev [8] has reported that, during November 1958, gases escaped from the region of the central peak of Alphonsus, and he published a spectrum of these gases. He states that the original spectrum shows the presence of the C_2 bands which are prominent features of comet spectra. No change in the appearance of the central peak has been noted, and thus this could not have been a typical volcanic eruption such as is observed on the Earth: possibly it was an eruption of water from the interior. Many small black areas have been observed in various parts of the Moon, and in fact several examples appear in Alphonsus. Often they have small craters in their centres and look as though gaseous eruptions of some kind had occurred. If C_2 — or rather, compounds which the solar light dissociates to give C_2 — escapes from the Moon, some blackening of the surface by graphite should occur.

A very curious and puzzling feature of the surfaces of the maria are the great sinuous wrinkles some one hundred metres high, some kilometres wide, and hundreds of kilometres long. G. P. Kuiper [9] has recently reported that there are often cracks along their tops, and in some cases white material can be seen in these cracks. Does the white material consist of encrustations of salt, possibly deposited by escaping water? J. W. Salisbury [10] has made an interesting suggestion in regard to some rounded hills that have been observed for many years. He suggests that water has caused hydration of olivine present in these regions, causing it to swell and elevate the surface. Possibly the wrinkles in the maria overlay cracks below, through which water rises from the interior. The wrinkles look much like sand dunes or slides of some kind. Their origin is uncertain, but probably they are superficial in origin, and the unravelling of the puzzle will contribute little toward a fundamental understanding of the Moon.

Long cracks occur in various regions of the Moon, and in some places craters are distributed along them. The curious distribution of the craters along these cracks show that they are certainly not due to collisions. They are due to the escape of gases from the interior, and have long been interpreted in this way. An outstanding example lies just west of Copernicus, but others exist in the land areas and in the regions near the centre of the Moon's disk. These cracks are probably due to some general phenomenon, such as a gradual expansion of the Moon due to increasing temperature; the escape of gases may well be due to this same circumstance.

The big valleys have not yet been discussed. The Alpine Valley is the most prominent of these and is about 130 km long. It points in the direction of the great collision in Mare Imbrium and has been ascribed to the effects of an iron-nickel object ploughing through the surface. It has also been suggested that it is due to a crack in the lunar surface. It is certainly much straighter than the other features, which all agree are cracks. The Rheita and Borda valleys in the south-west regions are even longer. All are remarkably straight; if they are in fact cracks ploughed out by iron-nickel objects it is evident that these were very large and were moving with high velocity.

SOME PROBLEMS OF LUNAR EXPLORATION

On all grounds, the most immediate task of lunar exploration should be to determine the composition and physical character of the maria. Quite simple observations on a few samples of material by methods that permit the study of the crystalline structure would unequivocally answer questions raised in this paper. Such observations cannot be made from a distance of 384 000 km: securing better photographs that will resolve smaller craters will give but little additional information. The colour of the rocks is determined by their composition and by the action of ultraviolet and particle radiation in a near-perfect vacuum during the last 4·5 aeons.

Radioactivity could be measured from a space vehicle flying some hundreds of kilometres above the surface. If the radioactivity is similar to that of the Earth, much differentiation of the lunar surface by melting processes must have occurred. If the concentration of radioactive elements is low, we must conclude that the composition is similar to that of meteorites and that little differentiation has occurred. The former result would mean that the Moon has had a very high-temperature history, and the latter that it has had a low-temperature history.

The Moon's density is less than that of the Earth, even when allowance is made for the compression in the latter due to its great interior pressures. This means that the Moon contains less iron than the Earth or more low-density substances, most probably water. If the reason is more water, this would be expected to be concentrated in the surface rocks as water of crystallization, and the percentages required are large— probably 10 per cent or more. This could be detected easily if simple chemical analyses could be made. If the reason for the low density is lack

of iron, this would indicate that the Moon has a composition similar to that of the Sun, and that the Earth and the other planets acquired increased amounts of iron during their formation. This bears on two important problems, namely, the origin of the elements and the origin of the Solar System.

The rigidity could be determined by seismographs and gravimeters placed on the surface, and these could be flown to and landed on the Moon. Such measurements will show whether the Moon is now molten in the deep interior and give much information about its internal temperature.

The determination of the age of the Moon's surface, by radioactive dating methods, will tell us definitely the time at which the events described in this paper occurred. The surface materials—particularly at the poles, which have remained cool during geologic time—will give us a record of cosmic-ray intensities during the time that the Solar System has existed. For such studies it is necessary to extract the inert gases from the surface materials. Knowing how rapidly they are produced by cosmic rays, we can calculate the time of exposure if the intensity of the rays has been constant. If the age so calculated agrees with the radioactive ages, we should then know that the cosmic rays have been constant in intensity, at least on the average. Disagreement would require another explanation.

The Moon has been an object for astronomical study for centuries. We are now entering a period when physical and chemical studies will supplement the astronomical ones. Certainly the petrologists will have a field day if at some time samples of the Moon can be secured. However, if the story of the Moon as presented here is correct, the usual processes of geology—the mountain uplift, volcanology, erosion, sedimentation, and formation of the fossil record—will have little application to selenology.

ACKNOWLEDGMENT

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The distribution of cellular matter during meiosis

A. LONGWELL and M. MOTA

For certain kinds of cytological work, interference microscopy has important advantages: not only does it make visible some parts of the cell that cannot be differentiated by staining, but it makes possible the measurement of the relative densities of parts of the cellular structure. This article describes a survey that has been made, using the interference microscope, of the distribution of matter in the cell during the process of meiotic division.

The relatively recent introduction of the interference microscope enables a new approach to be made to biological problems of long standing, as it makes it possible to study the distribution of dry matter in the cell during division. Whereas morphology in the past has been concerned with the linear dimensions, it is now possible to say something about the weight of the various cellular components.

A knowledge of the arrangement of mass during cell division is especially valuable in understanding the origin and formation of the mitotic spindle, which plays an important part in the separation of the chromosomes and their movement to the poles of the cell. Study of this spindle is difficult, because it cannot be stained with any of the dyes commonly used; moreover, under ordinary microscopic conditions, it is difficult to see its shape, and even, sometimes, whether or not it is present. However, D. Mazia and K. Dan have beautifully demonstrated the existence of the achromatic spindle as a separate cellular entity, and have learnt something of its chemical composition [1].

The fibrous nature of the spindle has long been noted, and W. J. Schmidt showed, by means of a polarizing microscope, that the spindle shows a positive birefringence along its longitudinal axis, and must therefore be composed of orientated particles [2]; this has been confirmed by S. Inoué [3]. Of the more recent tools, the phase-contrast microscope gave relatively little new information concerning the spindle. However, it demonstrated, especially in living cells, the shape of the space occupied by the spindle by showing the mitochondria arranged around it. J. M. Mitchison and M. M. Swann, and R. R. Rustad, have applied the interference microscope to a study of the spindle apparatus [4, 5].

Small differences in the dry mass of parts of individual cells show up, under the interference microscope, as differences in colour when white light is used, and as differences in intensity when monochromatic light is used. This is a consequence of the relationship between the concentrations of the solids in the cell and the phase changes undergone by the light that passes through the object [6]. The two-beam interference microscope translates the invisible phase-changes into intensity or colour differences, which are caused by an interference effect between two beams, one that has not passed through the object, and another that has passed through the object and has been modified by it. The phase difference between the two interfering beams can be varied, enabling one to measure the difference between the optical path through the object and that through the mounting medium. By varying the phase difference between the two beams it is possible to choose the type of contrast, of colour or intensity, best suited for a study of the particular detail being observed. The colour or intensity of a particular point in a cell as seen in the interference microscope depends on the difference between the product of the refractive index and thickness of that region on the one hand, and that of the mounting medium on the other. Appearance of the cell and contrast within the cell can be greatly altered by varying the refractive index of the mounting medium, so as to show maximum or minimum detail in either the nucleus or the cytoplasm. This is due to the fact that the reference beam, passing outside the object, is changed when the refractive index of the medium is altered. By matching the refractive index to that of any particular component in the cell, this cellular detail can be made to disappear. By changing the refractive index stepwise, an excellent differentiation of internal structure can be obtained.

FIGURES 1–6 – *Microsporocytes of* Zea mays *in* 5 *per cent acetic acid.*

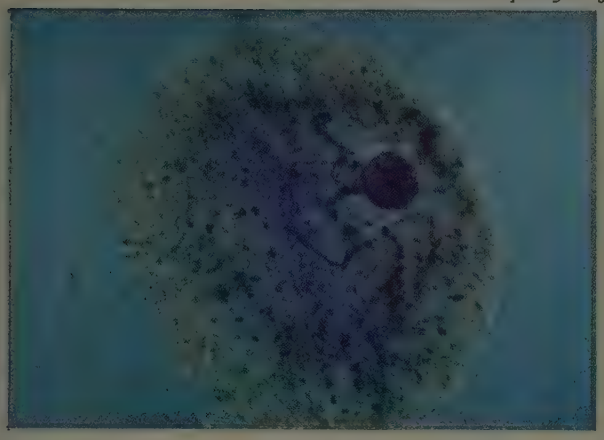


FIGURE 1 – *Mid-pachytene. The nucleus and chromosomes appear blue against the yellow cytoplasm. The nuclear sphere is also seen in blue. (All photomicrographs* × 1650–2000 *approx.)*

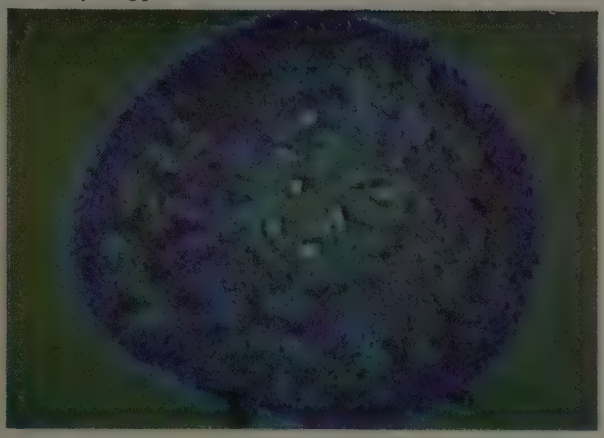


FIGURE 2 – *Prometaphase I. The chromosomes appear in green against a blue cytoplasm. They lie in scattered clumps which are differentiated from the rest of the cytoplasm by their greater density.*

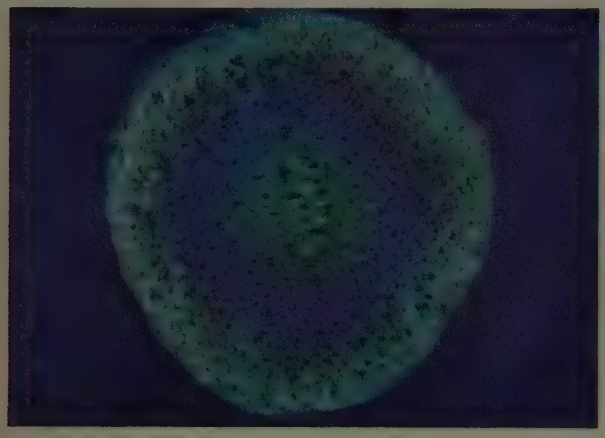


FIGURE 3 – *Metaphase I. Note the similarity in colour of the spindle fibres and the chromosomes. Since these regions are of approximately equal optical-retarding properties, both appear green. The spindle fibres appear to be double.*

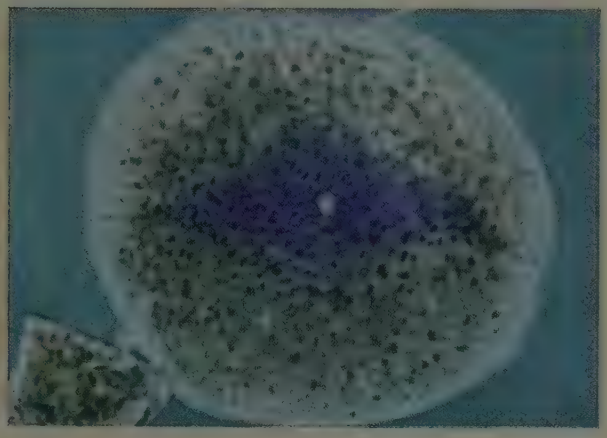


FIGURE 4 – *Metaphase I. The spindle, which shows in blue, is well differentiated from the cytoplasm, which appears white.*

FIGURE 5 – *Anaphase I. The spindle and chromosomal regions are the same colour, indicating equal densities. The spindle is clearly differentiated and appears to have two poles, one superimposed upon the other.*

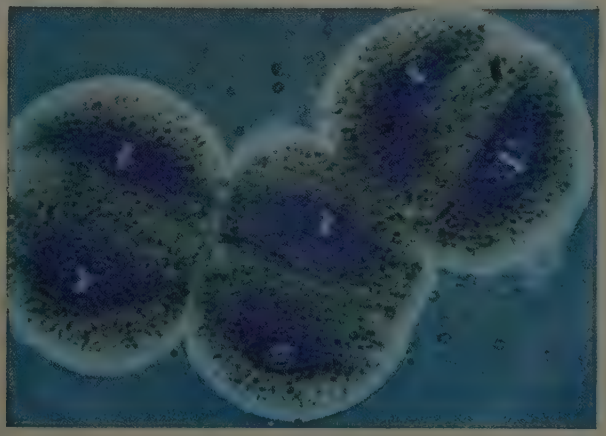


FIGURE 6 – *Metaphase II. The spindle again appears markedly differentiated from the chromosomes and the cytoplasm, and its fibrous nature is evident.*

FIGURES 7–18 – *Microsporocytes of* Triticum aestivum *in* 5 *per cent acetic acid.*

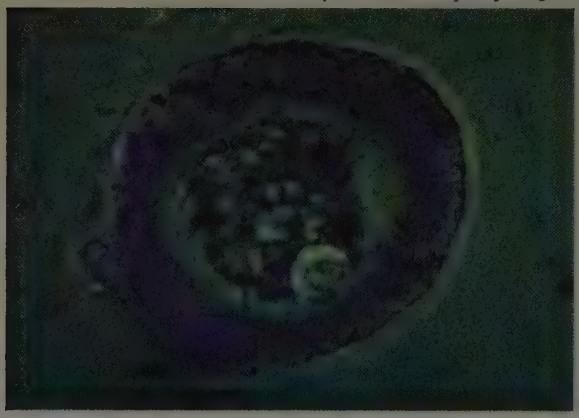


FIGURE 7 – *Early prophase I. The nucleolar region appears in yellow.*

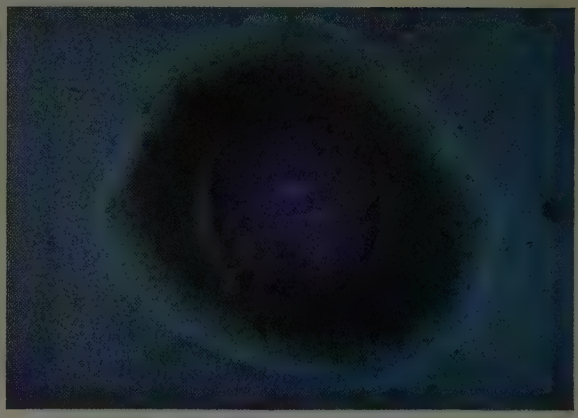


FIGURE 8 – *Mid-prophase I. The denser nuclear sphere appears dark against the green cytoplasm.*

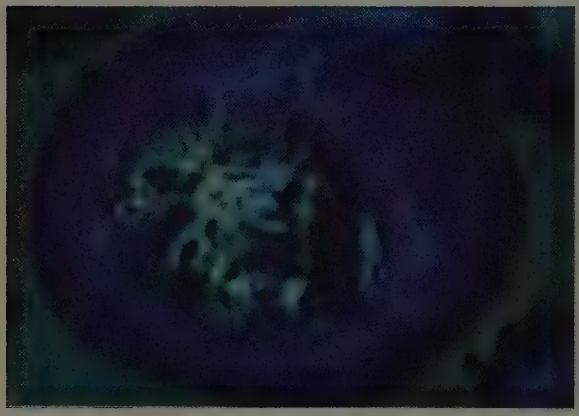


FIGURE 9 – *Late pachytene. The nuclear sphere is markedly differentiated from the rest of the cytoplasm.*

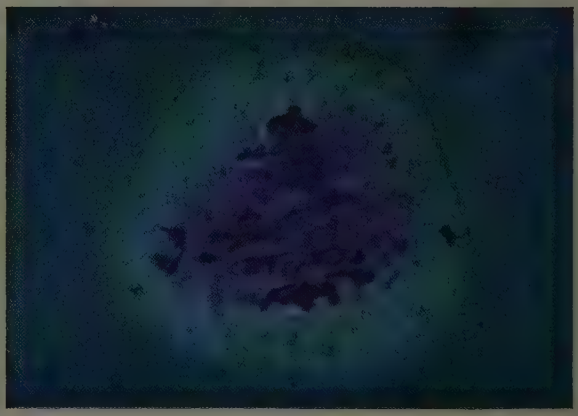


FIGURE 10 – *Metaphase I. The chromosomes appear almost as in an aceto-carmine-stained preparation.*

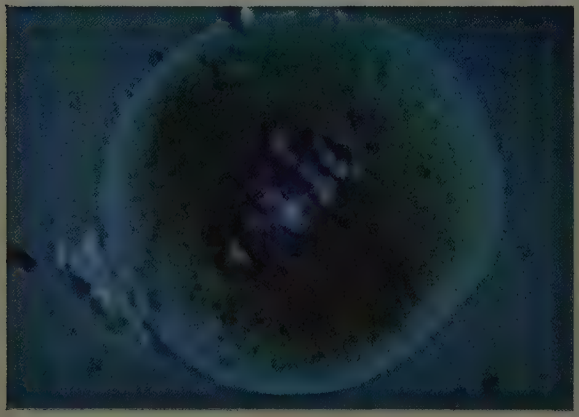


FIGURE 11 – *Metaphase I. The spindle region is differentiated from the rest of the cell, appearing dark against the light-blue cytoplasm.*

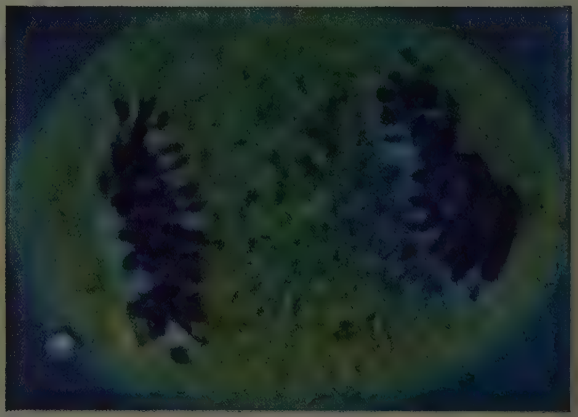


FIGURE 12 – *Early telophase I. The chromosomes appear almost as in a stained preparation.*

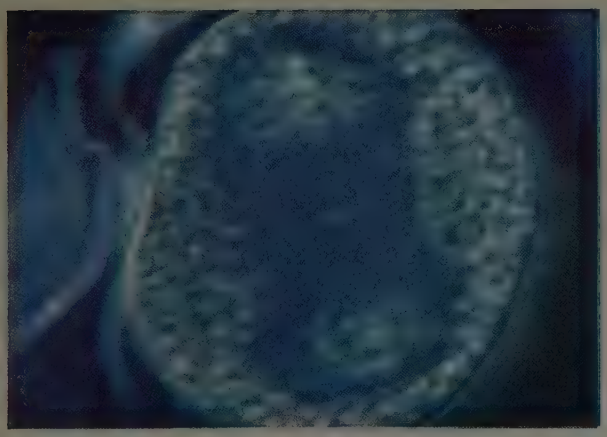


FIGURE 13 – *Telophase I. The region earlier occupied by the spindle is well differentiated from the remainder of the cytoplasm, being much more dense.*

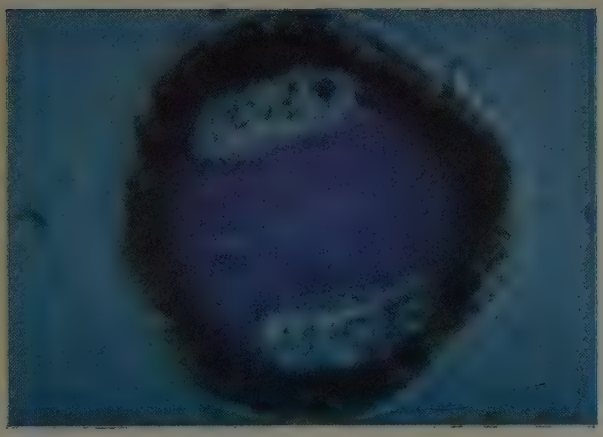


FIGURE 14 – *Telophase I. The nuclei appear almost as if stained.*

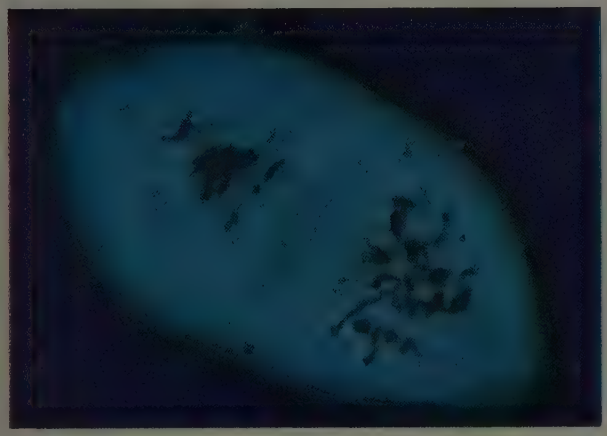
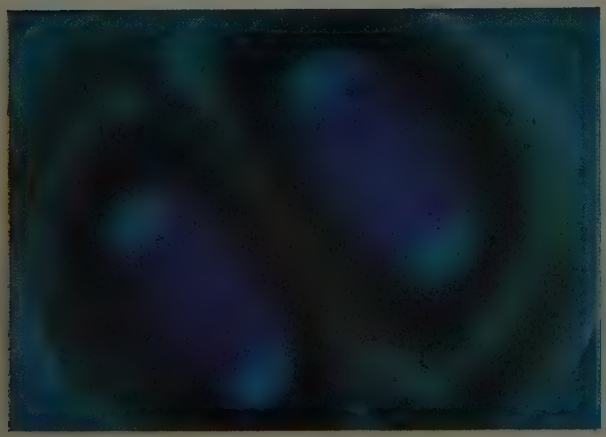


FIGURE 15 – *Prometaphase II. The chromosomes appear dark against a light-green cytoplasm.*

FIGURE 16 – *For description see figure 17 below.*



FIGURES 16 and 17 – *Early telophase II. The chromosomes appear markedly differentiated from the rest of the cell.*

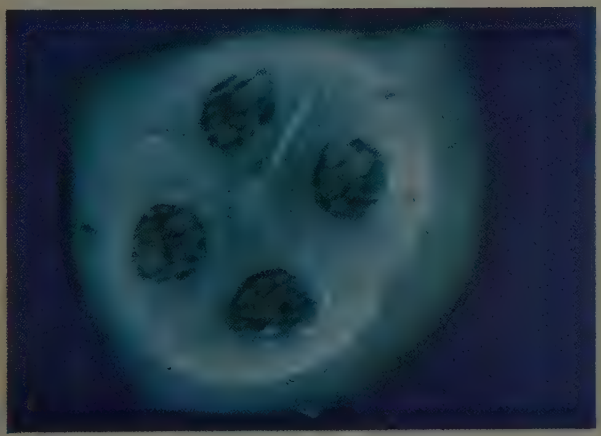


FIGURE 18 – *Quartet of microspores. The nuclei appear sharply distinguished from the rest of the cell.*

FIGURES 19–21 – *Spermatocytes of* Melanoplus differentialis *in physiological saline solution.*

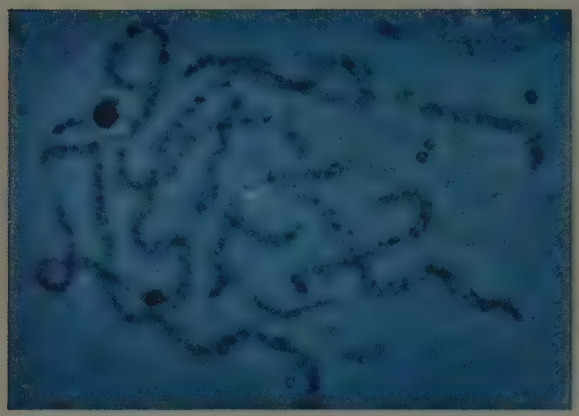


FIGURE 19 – *Pachytene. The heterochromatic, or condensed and compact, sex chromosomes are visible. The chromosomal regions appear light or dark, depending upon their densities or mass.*

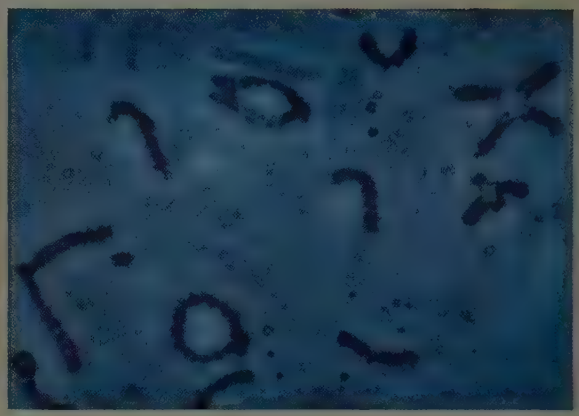


FIGURE 20 – *Diplotene. Some of the bivalents show crossing-over, or the interchange of portions of two chromatids while the other two chromatids remain intact.*

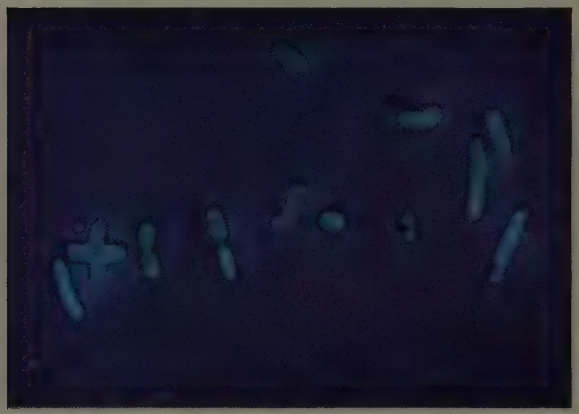


FIGURE 21 – *Metaphase I.*

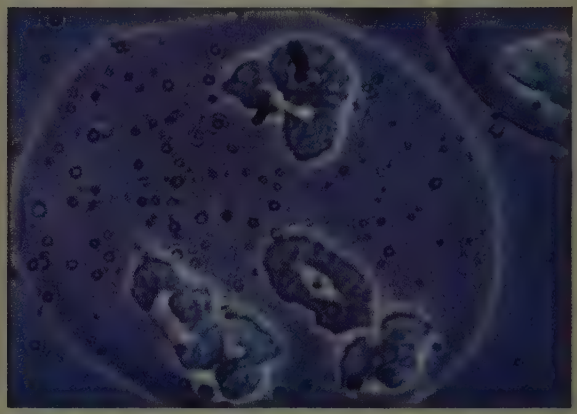


FIGURE 22 – Tradescantia paludosa *microsporocyte at prometaphase I. The major coil in the chromosomes is evident.*

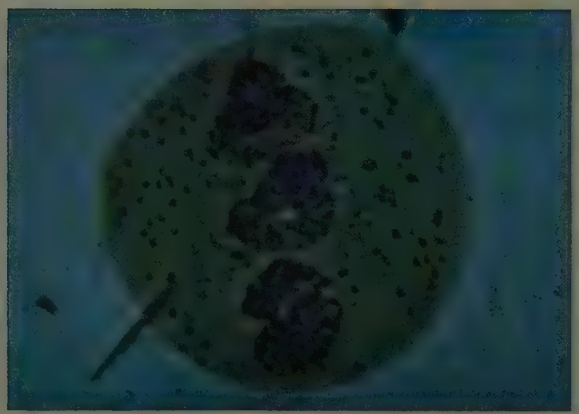


FIGURE 23 – Tradescantia paludosa *microsporocytes at metaphase I. The major chromosomal coil shows clearly.*

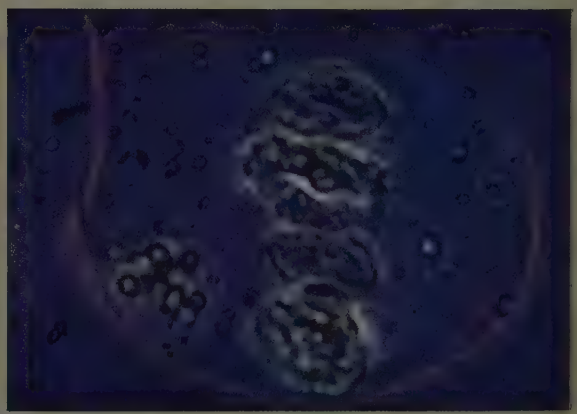


FIGURE 24 – Tradescantia paludosa *microsporocyte at metaphase I. The spindle attachment regions show as projections from the chromosomes, and appear in the same colour as the chromosomal coils.*

THE STUDY OF MEIOSIS

Using the interference microscope, the various regions of the cell can be investigated by means of the colour differences they show in unstained preparations; again, structures normally invisible even in stained preparations will appear in different colours. The colour patterns observed in the cells are roughly indicative of differences in dry mass. Meiosis was studied in the microsporocytes of plants of common bread wheat *Triticum aestivum*; maize *Zea mays*; the spiderwort *Tradescantia paludosa*; and in the spermatocytes of the grasshopper *Melanoplus differentialis*.

Meiosis is a modification of mitosis, the normal process of cell division: it is a division that results in the formation of male or female gametes, each with a chromosome number half that found in the cells of the rest of the organism. Prophase, the first stage of meiosis, can be divided into five stages: leptotene, zygotene, pachytene, diplotene, and diakinesis.

At leptotene, the chromosomes appear as fine threads and cannot be individually distinguished. The threads appear to be single and resemble strings of beads, because of the presence of dense granules, called chromomeres. In early and middle prophase, one or more dense bodies, known as nucleoli, are evident in the nucleus (figure 7). At zygotene (figure 8), the two corresponding chromosomes from each parent (the homologous chromosomes) move together, and become paired along their entire length: these associated pairs of chromosomes are called bivalents. During pachytene (figures 1, 9, 19), the paired chromosomes become shorter and thicker, and the two chromosomes of each bivalent become twisted about one another. At about the middle of this stage, a longitudinal cleavage forms in each homologue, perpendicular to the plane of the pairing; the two homologous chromosomes of each bivalent now consist of two chromatids, the entire bivalent thus being composed of four chromatids. Transverse breaks may occur at the same level in two homologous chromatids, and chromatid interchange and fusion may occur. This process is called crossing-over (figure 20), and results in the interchange of genes. At the next stage (diplotene, figure 20) the paired chromosomes begin to repel one another and to separate, although the homologous chromosomes remain united at their points of interchange. The chromosomes then become greatly contracted, the nucleolus begins to disappear, and the nuclear membrane breaks down (diakinesis). This period, just before the formation of the mitotic spindle, is called prometaphase (figure 2).

Next a spindle is formed, and the bivalents move to the centre of the cell. The spindle-attachment regions of the two chromosomes (kinetochores, figure 24) comprising each bivalent come to lie in the equator of the spindle (metaphase, figures 3, 4). At anaphase, the two kinetochores of each bivalent, which have remained undivided, move away from one another and towards opposite poles of the cell, each kinetochore taking half of the bivalent with it (figure 5). When the chromosomes reach the poles of the spindle they become uncoiled (telophase), and two new nuclei, and subsequently daughter cells, are formed.

After an interphase, these two nuclei pass into the last part of meiosis, known as the second division. This division separates the two chromatids of each chromosome. These chromatids, though identical in their formation, differ in those segments which have been exchanged by crossing-over. Telophase passes into prophase, and the chromosomes contract, thicken, and become more distinct. They can be seen as pairs of threads which are held together by an undivided kinetochore. A spindle appears in each new cell, and the chromosomes become arranged on the equator of the cell. The kinetochores then divide, and the daughter chromosomes move to the poles. At second telophase there are four nuclei, each with the haploid number of chromosomes, which eventually become separated from one another by new cell walls (figure 18).

Studies with the interference microscope showed that, from middle prophase to late prophase of the first meiotic division (middle prophase 1 to late prophase 1) in wheat and corn, the nucleus was surrounded by a differentiated spherical zone. This zone was described by F. Wassermann in 1929 [7], and may be similar to the clear zone described by A. Bajer [8] in living endosperm cells; it can be seen in figures 1, 8, and 9. This region seems to grow until it breaks down at late prophase or prometaphase, the latter being the stage just before full metaphase, that is, before the arrangement of the chromosomes in the equatorial plane of the cell. The sphere appears to move toward the nuclear membrane (figures 8, 9) during prophase. There is no concentration gradient between the nuclear membrane and this region, as would be expected should the material of which it is composed be coming from the nucleus. It is possible, then, that it is cytoplasmic in origin, and that it approaches and touches the nuclear membrane in late prophase as a result of growth of the sphere, growth of the nucleus, and possibly even

by movement of this region towards the nucleus. In prometaphase I, in corn, the chromosomes lie in a part of the cell differentiated from the rest of the cytoplasm, and only remnants are evident of the sphere which at earlier stages had surrounded the nucleus (figure 2). It is possible that the material forming the sphere gives rise to this mass, or at least makes a substantial contribution to it, and that the spindle then arises from this region. There is no evidence of fibres in this region of the cytoplasm at prometaphase. Wassermann [7] first suggested that the spherical region contributed to the formation of the spindle.

When the chromosomes arrange themselves in the equatorial plane, the spindle, as seen in observations on wheat and corn, appears to be composed of double fibres (figure 3), some of them being attached to the chromosomes (figures 3–6, 11). In *Zea*, where the chromosomes are relatively small compared with the size of the cell, the spindle in late metaphase or in mid-anaphase appears to show two poles on either side of the equatorial plane, as if a shorter and a longer spindle were superimposed (figure 5): the chromosomes appear to reach only the nearer pole and do not move on to the farther pole. In late anaphase the region between the two groups of daughter chromosomes also shows fibres. From this stage on to late telophase, the region between the two daughter nuclei becomes increasingly differentiated from the rest of the cytoplasm (figures 13, 14), as well as from the region behind the telophase nuclei which corresponds to the more distant pole of the spindle (figure 14). In the equator of this region a thin band later develops and forms the new cell wall (figures 16, 18).

It is possible, by increasing the refractive index of the medium until it approaches that of the cytoplasm, to make the cytoplasm practically invisible, and the chromosomes are then much better defined. In *Tradescantia* (figures 22–24), the major coil of the chromosome is clearly seen. The kinetochores, the points of attachment of the spindle fibres to the chromosomes, are well defined and quite conspicuous, and have the same optical retarding properties as the chromosomal coils.

Diplotene is the period when the homologous chromosomes start to repel one another and separate. The four chromatids—the two filaments composing each of the two chromosomes—are so clearly seen in grasshopper spermatocytes that crossing-over can be analysed at least as well as in stained preparations (figure 20). The advantages of this technique are that the densities of the various parts can be determined and that they appear in a colour different from that of the surrounding medium, making differentiation much easier. The chromosomes at pachytene, the stage at which there is complete longitudinal pairing of the homologous chromosomes, and at metaphase, are also seen as clearly as in stained preparations (figures 19 and 21). *Triticum* chromosomes, again, appear the same as in stained preparations, showing that no important artifact is introduced by staining. Likewise the nucleolus, the refractile spherical body in the nucleus, can be made to stand out from the rest of the cell (figures 1, 7). The spindle region can be made to disappear, by adjustment of the analyser, so that the chromosomes are more clearly seen (figures 10, 12, 15, 16).

The optical-path retardation of the light through the various regions, as shown by differences in intensity, was measured in *Zea mays*, using monochromatic green light (5460 Å); the material was mounted in distilled water and in 15 per cent acetic acid. The regions measured were those occupied by the cytoplasm, chromosomes, nuclear sap, nucleolus, the sphere surrounding the prophase nuclei, the region of the cytoplasm in which the chromosomes lie at prometaphase, and the spindle in metaphase, anaphase, and telophase. Although the absolute values of the densities of these regions vary considerably from cell to cell, their order in any one cell was always the same.

The actual retardation of light lay approximately between one-half of a wavelength and two wavelengths when distilled water was used as a mounting medium. The region of the cell through the nuclear sap is the only region with less mass per unit volume than the cytoplasm, in agreement with K. F. A. Ross's finding [9] that this region has the lowest refractive index. The sphere around the nucleus in prophase I is always denser than the rest of the cytoplasm. The nucleolar region is denser than the sphere, and the chromosomes are the most dense structures. The retardation of light passing through the regions of the cell occupied by them could be as much as one wavelength more than that of light passing through the cytoplasm alone. The part of the cell in which the chromosomes lie in prometaphase I is denser than the cytoplasm, although less dense than the chromosomes.

In metaphase of both first and second divisions, the spindle region is more dense than the cytoplasm, but less dense than the chromosomes. The cytoplasm of sea-urchin eggs was found by Mitchison and Swann to have a refractive index equal to or higher than that of the spindle and

chromosomes, which may be attributed to the fact that the cytoplasm of these cells is particularly rich in storage material [4]. The spindle region between the chromosome groups is also denser than the cytoplasm in both anaphase I and II, and the density of that region is still higher in telophase. This increase in the density of the region between anaphase I and anaphase II is in good agreement with Rustad's findings, and with the fact that in some cases ribonucleoproteins have been detected in that region after anaphase [5, 10]. Wassermann has described the release of a substance from anaphase chromosomes [7]. Should the increase in density be produced by material released from the chromosomes, it supports the hypothesis that explains movement of the chromosomes in anaphase by the release of some substance from the chromosomes [11].

The relations between the densities of the different regions are in all probability identical in all cells, and in several hundreds of cells observed without measurement it was possible to verify this simply by noting the differences in colour. Absolute values for the retardation caused by the various cellular structures can be obtained either by isolating the component and then measuring its optical retardation, or by observations of intact cells when there is a good knowledge of the geometry of the parts of the cell. At mid-pachytene the nucleolus in *Triticum aestivum* retarded green light by about one wavelength. The total dry mass of the nucleolus was calculated to be about 3×10^{-11} g.

The facts that light retardation was greatest through the regions occupied by the chromosomes and that these are the smallest structures studied indicate that the chromosomes are the densest structures in the cell. Likewise the density of the other structures is certainly arranged in the order given above. The larger the structure, the smaller is the amount of cytoplasm above and below, and so the greater is the retardation caused by the structure alone.

Interference microscopy may also make possible further studies of chromosomal duplication. An analysis of the distribution of mass in the chromosomes could be made, and maps could be constructed showing the distribution of dry matter.

When utilized at its maximum sensitivity, and when the technical difficulties presented by different biological materials are overcome, interference microscopy makes it possible to detect light-retardation of not more than one-tenth wavelength. This corresponds to a dry mass of 3×10^{-13} grams per square micron when monochromatic green light of wavelength 5460 Å is used.

ACKNOWLEDGMENTS

This study was made at the Argonne National Laboratory under the auspices of the U.S. Atomic Energy Commission, and partly financed by a grant to M. Mota from the National Academy of Sciences (Washington) and the International Cooperation Administration. The senior author thanks Mr George Svihla of the Argonne National Laboratory for assistance with the interference microscopes.

TECHNICAL DETAILS

The material was fixed in Carnoy's solution (ethyl alcohol, chloroform, and glacial acetic acid) for at least two days and then passed to 45 per cent acetic acid and carried down to distilled water. Smears were made in concentrations of acetic acid ranging from 45 per cent to 5 per cent, and in distilled water. The slides were sealed to avoid evaporation of the mounting medium.

Illumination was obtained from a high-pressure mercury-vapour lamp. No filter was used for general observation and in taking pictures showing colour separation of the different cell regions. Measurements were made using an interference filter to isolate the 5460 Å green light. To obtain optimum conditions for measurement and observation, the first-order white fringe was spread out to cover the whole field of view so that the phase of the background was constant for any setting of the analyser. The optical path difference was measured by reading the number of degrees through which the analyser had to be turned from the position of maximum extinction for the background to the corresponding position for maximum extinction for the region of the cell being measured. Both the Cooke-Dyson and the AO-Baker interference microscopes were used. To determine the number of wavelengths by which light was retarded across the object, the Cooke-Dyson microscope was used with the fringes in the field of view, and colour fringe matched to colour fringe.

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The plant cytochromes

W. O. JAMES and RACHEL M. LEECH

A great deal of work has been concentrated on the nature and biological function of the cytochrome pigments, which occur in all aerobic organisms. They have long been recognized as cell catalysts of the highest importance, and recent investigations of those found in plants has enlarged the limits of the group and suggested new functions for some of its members.

The cytochrome pigments occur in the cells of all aerobic organisms. Because of their ready fluctuation within the cell between the ferrous and the ferric states, they are all efficient biological electron-transporters and play a vital role in cellular oxidations in both plants and animals. They are generally regarded as the well-nigh universal catalysts of respiration, forming the essential electron-bridge between the respirable substrates and oxygen.

The cytochromes are conjugated proteins in which the catalytically active prosthetic group is an iron porphyrin. They can readily be converted to corresponding haemochromogens by replacement of the protein with an organic base, usually pyridine. Ferrous cytochromes show a typical absorption spectrum with three maxima only slightly less intense than those of the pyridine haemochromogen itself. The three maxima vary with the specific pigment, but lie within the ranges $\alpha = 550–605\ m\mu$, $\beta = 520–530\ m\mu$, $\gamma = 415–450\ m\mu$. The relative intensities of the bands are $\gamma \gg \alpha > \beta$: the γ-peak is about 4 times higher than the α. On oxidation, the α- and β-peaks are replaced by a diffuse zone of absorption and the γ-peak is displaced somewhat towards the blue. The change of absorption when a cytochrome is reduced may be very slight or even undetectable at the position of the β-peak, but changes at the α-position may be diagnostic.

Knowledge of the occurrence and biological behaviour of the cytochromes still rests very heavily on these spectral characters and dates from a paper published by D. Keilin in 1925 [10] that revised and extended the earlier observations of MacMunn. Keilin showed that the typical cytochrome spectrum was observable in a very wide range of plant as well as animal tissues. The typical spectrum consisted of four bands readily detected with a low-dispersion visual spectroscope (figure 1). Three of the four bands were interpreted as the α-bands of cytochromes a, b, and c and the fourth as their unresolved β-bands. It was later decided that cytochrome a made no measurable contribution to the β-bands. Such spectra were shown by numerous plant tissues after reduction with dithionite, the bases of shallot (*Allium ascalonicum*) bulbs being particularly favourable subjects. Figure 1 reproduces Keilin's original diagram comparing the spectra observed with shallot bulbs and yeast. The range of observations has been considerably extended by later workers [for example 2, 3, 16, 18].

The typical spectrum, sometimes with additions, has been seen in members of sixteen families of flowering plants. It is recorded from almost all the organs of plants, including pollen, embyros, leaves, roots, flowers, and fruits, and from specialized tubers and the spadix, the sterile club of the *Arum* inflorescence. Cytochromes have also been identified spectroscopically in the green algae, *Chlorella*, *Chlamydomonas*, *Euglena*, *Vaucheria*, *Caulerpa*, and *Ulva* and in all the main groups of the fungi [3]. There is presumptive evidence of their occurrence in fern spermatozoids [17].

Plant tissues present two difficulties which make the observation of cytochromes in them less easy than in others. The concentration of cytochrome in plant cells is very low and has been calculated to be of the order of 10^{-6}M [8], although concentrations in yeast may be as much as ten times higher. Plant tissues also contain interfering pigments in troublesome concentrations. Since the time of Keilin, observers of plant tissues have nearly always found a diffuse band of absorption lying between the wavelengths 550 and 560 mμ under both oxidizing and reducing conditions (figure 1, bottom row). On reduction, the bands due to cytochromes b and c then appear only as intensifications of its two edges instead of as two independent bands.

The application of spectrophotometers of the type recently described by H. Lundegårdh [19] has advanced the study of the plant cytochromes

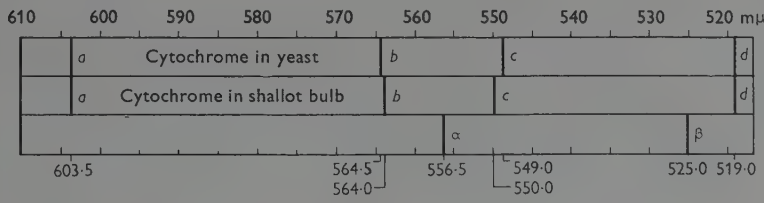

FIGURE 1 – *Position of the visible bands of cytochromes a, b, and c in yeast and shallots, and the bands observed in runner beans. Reproduction of Keilin's diagram* [10]. *Red end of spectrum on left.*

very considerably during the past decade. By these and other means it has been shown that cytochromes a, b, and c are not single substances but are each typical of groups. For example, the α-peak of cytochrome b observed visually in young barley roots at about 561 mμ can be resolved under favourable conditions into the α-peaks of cytochromes b and b_3 at 563 and 560 mμ respectively (figure 2).

Cytochromes a, b, and c are each assumed to be identical, or at least closely similar, whatever their source, whether from plant or animal species. About a score of cytochromes are now known, and, of these, four are at present known only from plant sources. Table I lists all those, nine in number, that have been identified in plants, and gives their principal diagnostic properties.

The plant cytochromes may be divided into three main groups, namely c, c_1, f; b, b_3, b_6, b_7; a and a_3. The members of each group have similarly placed α-absorption peaks in the reduced state, give rise to similar pyridine haemochromogens, and have a similar behaviour towards oxygen (see table I).

CYTOCHROMES OF THE C GROUP

The α-peaks of these cytochromes lie between 550 and 555 mμ. Their prosthetic group is protohaematin (iron protoporphyrin), but it is bonded to the protein by way of two thiol groups of the protein and the two vinyl groups of the porphyrin. Separation of the two parts of the molecule leaves the thioether link intact so that part of the protein remains attached to the protohaematin.

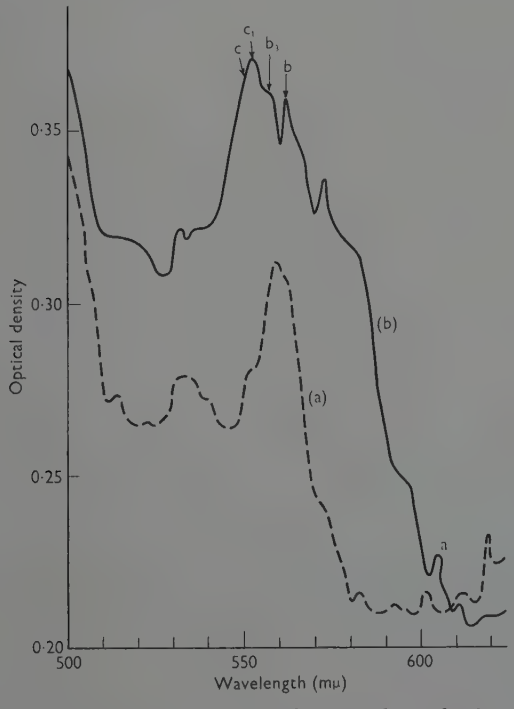

FIGURE 2 – *Absorption curves of a 17 mm layer of 3-day-old barley roots under aerobic* (a) *and anaerobic* (b) *conditions* [20].

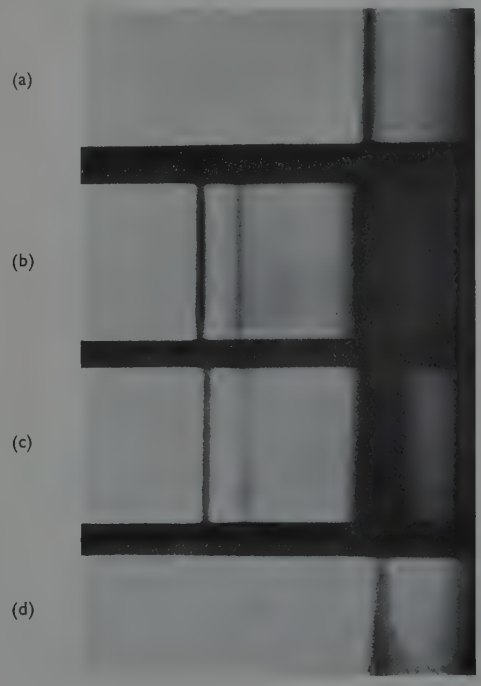

FIGURE 3 – *Absorption spectra.* (a) *Cytochrome f (from parsley leaves), showing only a γ-band.* (b) *The same, but concentration 6 or 7 times greater showing α- and β- bands;* (d) *and* (c) *Cytochrome c (from horse heart) at the same two concentrations. Red end of spectrum on the left.* [21].

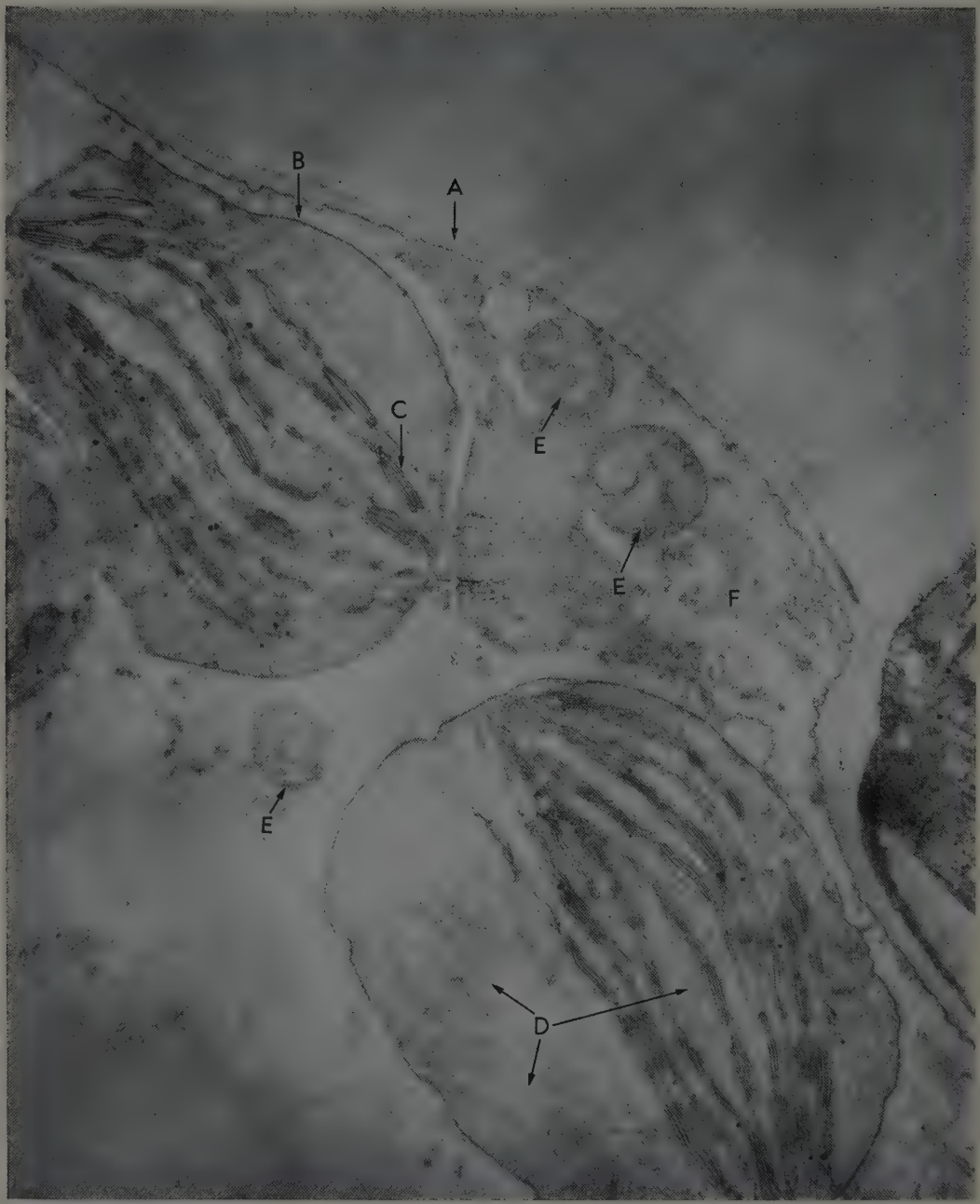

FIGURE 4 – *Part of a cell from the spongy mesophyll of a broad-bean leaf. A, cell wall; B, chloroplast; C, granum with lamellae cut transversely; D, stroma of chloroplast; E, mitochondria, showing tubular infoldings of the wall; F, cytoplasm. (Electron micrograph by M. S. C. Birbeck.)* (× 22 000)

TABLE I

Some properties of plant cytochromes

Cytochrome	Source of partially purified cytochrome	Absorption peaks of reduced form (mμ)			α-peak of pyridine haemochromogen	Standard redox potential E'_0 (volts)	Reaction with			Solubility	References
		α	β	γ			CO	Cyanide	O_2		
a		603	*None*	*452*	585	+0·29	No	No	No	Insoluble	[6, 11]
a_3		ca. *600*	*None*	*448*	585		Yes	Yes	Yes	Insoluble	[6, 11]
b		563	*530*	*432*	558	−0·04	No	No	Very slightly	Insoluble	[6, 11, 1]
b_3	*Vicia faba* leaves	560	529	425	557	+0·04	No	No	Yes	Soluble	[8, 13]
b_6		563	528	430	558	−0·06	No	No	Yes*	Insoluble	[7, 9]
b_7		560	529			−0·03	No	No	Yes	Insoluble	[6]
c	Wheat germ	550	521	417	551	+0·255	No	No	No	Soluble	[6, 8]
c_1	(Heart muscle)	*553*	*524*	*418*	551	+0·22	No	No	No	Soluble	[12, 22]
f	Parsley	555	526	422	551	+0·365	No	No	No	Soluble	[11, 6]

The wavelengths in italic figures have been verified for animal material only.
* Autoxidizable in chloroplast preparations.

All members of the group form the same pyridine haemochromogen with an α-peak at 551 mμ. They are readily extractable into aqueous solution and are much more stable than the other plant cytochromes. They do not react with oxygen under normal biological conditions, although cytochrome c will react with atmospheric oxygen above 70° C and is not denatured by the high temperature.

Cytochrome c. This has been found in every aerobic plant so far examined. The first extraction from a plant was achieved in 1935 by E. Yakashuji using the red seaweed *Porphyra tenera* [18]; but much purer products have now been had from wheat germ [5] and from the rust *Ustilago sphaerogena* [15]. Cytochrome c from either of these sources is identical in its catalytic activities with cytochrome c from heart muscle. The spectra of pure cytochrome c from the different sources are also identical, except that the γ-peak was reported to lie at 417 mμ for the wheat sample compared with 415 mμ for those from *Ustilago* and heart.

There are, however, some differences in the properties of the cytochrome c obtained from different sources. Whereas acid extraction media are used for animal material, only alkaline media are effective with plants; dilute NaOH was used for *Ustilago* and alkaline 0·1M K_2HPO_4 for wheat germ. Again, the *Ustilago* cytochrome c is reported to have a higher molecular weight (18 000) than heart or baker's-yeast cytochrome c (12 000) and a lower isoelectric point. These similarities and differences taken together suggest that, while the haem part of the molecule is the same in cytochrome c from different sources, the proteins to which the haem is attached may differ.

Cytochrome c_1. When a reduced heart-muscle preparation is cooled to the temperature of liquid air and viewed with a microspectroscope, a new band not visible at room temperature is seen at 552 mμ. This band has been shown to be due to the presence of a pigment, very like cytochrome c, that has been named cytochrome c_1. It has been extracted from heart muscle and in the reduced state shows peaks at 553, 524, and 418 mμ.

A similar band appears at 552 mμ in the spectrum of reduced preparations of the yeast *Torulopsis utilis* and of a large number of other fungi, at the temperature of liquid air [12, 3]. The new band has been interpreted as being due to cytochrome c_1. In the spectra of some reduced mitochondrial preparations from higher plants it has been noticed that the α-peak due to reduced cytochrome c is not at 550 mμ but has shifted 2–4 mμ towards the red end of the spectrum; in the mitochondria of wheat and skunk cabbage (*Symplocarpus foetidus*) the peak is at 553 mμ. A similar distortion is noticed in the reduced spectrum of wheat and of three-day-old barley roots (figure 2), where the α-peak is at 552 mμ. These distortions in the reduced spectrum have also been interpreted as indicating the presence of c_1.

Cytochrome f. This is found in the green cells of higher plants, and in green algae and *Euglena*. In the higher plants it is restricted to the chloroplasts, and it has not been found in animals or bacteria. It was extracted from elder (*Sambucus nigra*) leaves into ammoniated 96 per cent ethanol and highly purified by H. E. Davenport and R. Hill [4], who have described its properties in considerable detail. It is very similar to cytochrome c. Both have similar spectra, the absorption peaks of cytochrome f lying 4–8 mμ nearer the red end of the spectrum (figure 3). Both substances form the same pyridine haemochromogen and neither reacts with carbon monoxide or oxygen under normal biological conditions; both are oxidized by ferricyanide. Cytochrome f, although stable in comparison with most other cytochromes, is not as stable as cytochrome c. Below *p*H 4 and above *p*H 11 it is irreversibly denatured above 50° C. The redox potential of cytochrome f (+0·365 V) shows it to be the most oxidizing of all known cytochromes.

CYTOCHROMES OF THE b GROUP

The cytochromes of this group yield normal protohaemochromogens without the modification due to special sulphur bondings typical of the c group. The α-absorption peaks lie within the range 559–564 mμ; so far as plant cytochromes are concerned, there is no overlapping of the peak values of either the c or a groups. The b cytochromes are much less soluble, or at least extractable, than those of the c group and their proteins are more easily denatured.

Cytochrome b_3. Of the four plant members of this group, only cytochrome b_3 has been satisfactorily extracted and to a large extent purified. It appears to be present in many plant tissues but has been extracted only from broad bean (*Vicia faba*) leaves. The best preparations obtained still showed some contamination with peroxidase. This was revealed spectroscopically, after reduction, as a shoulder on the absorption curve at 594 mμ. Cytochrome b_3 is slightly soluble even in saturated ammonium sulphate, a property which was used to separate it from accompanying cytochrome c. It is denatured by cold acetone and acids. It reacts with oxygen at room temperature, but not appreciably with carbon monoxide.

The other cytochromes of the b group are held much more tenaciously in the cell structure, from which they have not yet been separated.

Cytochrome b. This is usually the most abundant cytochrome in plant cells, with a concentration about twice that of cytochrome c. It is immediately destroyed by cold acetone. Among the b cytochromes it is the least autoxidizable, reacting only very slightly with oxygen and not at all with carbon monoxide.

Cytochrome b_7. This compound, on the other hand, reacts extremely readily with oxygen, although, like the other members of this group, it does not combine with carbon monoxide or cyanide. In this respect b_7 is unlike the other highly autoxidizable cytochrome (a_3) which reacts with oxygen and carbon monoxide competitively. Cytochrome b_7 is so far known to occur only in the mitochondria of *Arum* spadices, in which it is found in relatively large amounts.

Cytochrome b_6. Unlike the other members of the b group, this is not denatured by cold acetone, which can therefore be used to facilitate its study after removal of acetone-soluble pigments such as the chlorophylls.

CYTOCHROMES OF THE a GROUP

This group has proved the most difficult to investigate in plants, because it has the lowest concentration of all and appears to lose its activity rapidly in homogenates at room temperature. Nevertheless, numerous visual observations of its α-band in plant materials, varying in position from 600 to 605 mμ, have accumulated. Recently it has also been recorded spectrophotometrically by several sets of investigators, but still with an equally wide scatter of the α-band. The γ-band at 444 mμ has also been recorded for wheat mitochondria as a shoulder on the large and composite peak in this zone.

Cytochrome a_3. This has recently been shown to be present in wheat embryos, where the α-band of the $a+a_3$ complex appears unusually strong relative to the b- and c-bands. Mitochondria extracted from wheat germ and then disintegrated by ultrasonic vibration and suspended in glycerol have proved amenable to examination in the spectrophotometer. The continuous curve in figure 5 shows the absorption by mitochondria reduced with dithionite. The location of the α-peak of cytochrome $a+a_3$ is shown at 602 mμ, with a secondary peak at 594 mμ characteristic of reduced peroxidase. After treatment with carbon monoxide, the absorption curve was transformed into that shown by the broken line, revealing the formation of the compound of cytochrome a_3 with carbon monoxide with an absorption maximum at about 590 mμ. At the same time, a corresponding change occurred in the region around 450 mμ. A similar change in this region only has

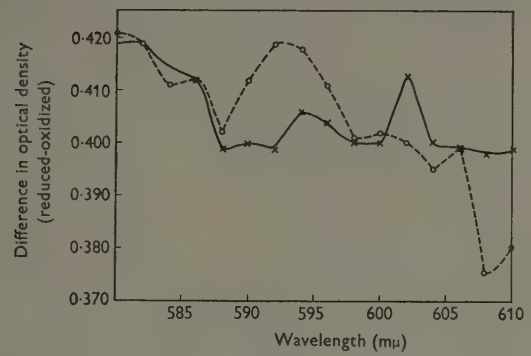

FIGURE 5 – *Absorption curves of a suspension of ultrasonically disintegrated mitochondria from wheat embryos. Solid line: difference spectrum, reduction with dithionite alone; broken line: reduced suspension treated with carbon monoxide.*

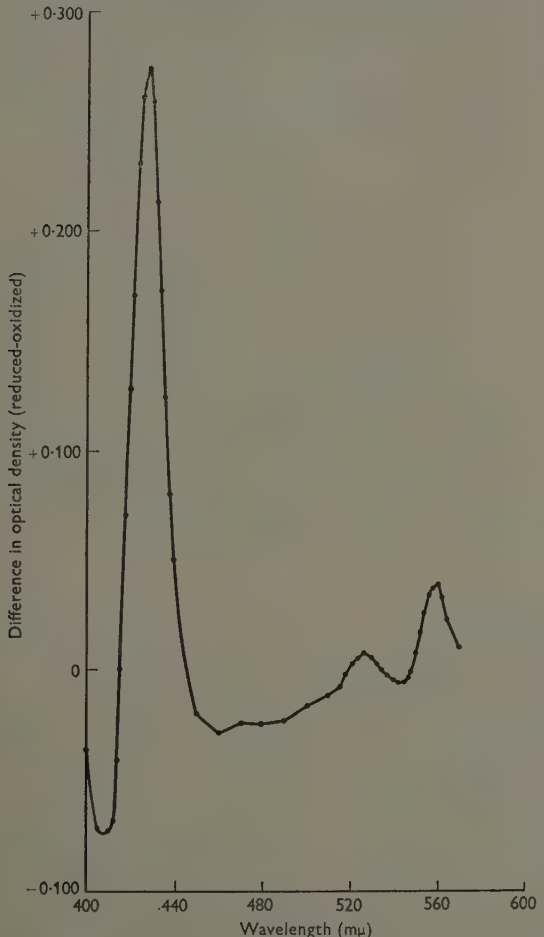

FIGURE 6 – *Difference spectrum (reduced-oxidized) of an acetone powder of grana from a purified preparation of chloroplasts from broad-bean leaves. The peaks due to cytochrome b_6 are displaced slightly to the left owing to the presence of some cytochrome f.*

been recorded for the mitochondria of skunk cabbage. The appearance of a band at 592–595 mμ, after carbon monoxide treatment, has also been observed visually with particles from the fungus *Neurospora crassa.*

INTRACELLULAR DISTRIBUTION OF THE CYTOCHROMES

Among recent developments the study of the localization of cytochromes within the cell is playing an important part.

Highly purified suspensions of chloroplasts from bean leaves and from the green alga *Caulerpa prolifera* have been examined with a spectrophotometer with photomultiplier attachment. Comparison under oxidized and reduced conditions showed that cytochromes f and b_6 were present and no others [9]. Contamination of the preparations with very small amounts of mitochondria introduced bands of cytochrome c. Owing to the relatively low absorption of chlorophylls in the green region, and the high sensitivity of the method used, it was possible to make the examination with normal chloroplasts without prior removal of the chlorophylls. The results confirmed those of previous investigations with etiolated barley chloroplasts, and colourless powders prepared from chloroplasts extracted with acetone. In such powders a band can usually be seen at 555 mμ, due to the presence of cytochrome f in a partially reduced state. On reduction with dithionite, the band at 555 mμ increases a little in intensity, but a new and higher peak appears at 563 mμ together with a lower peak at 532 mμ. The new absorptions represent the α- and β-bands of reduced cytochrome b_6.

Because they are restricted to chloroplasts and have theoretically favourable redox potentials it has been suggested that cytochromes f and b_6 function in photosynthesis in an electron transport system with chlorophyll. Direct observation of shifts in absorption, which would indicate changes in the state of the cytochromes, is difficult and, as yet, the direct participation of these cytochromes in photosynthesis has not been unequivocally demonstrated. It is, however, of some interest in this connection that we have recently been able to show that the cytochromes within the chloroplasts are associated very closely with the chlorophyll. When whole chloroplasts are disintegrated by ultrasonic vibration, the straw-coloured proteinaceous stroma can be removed from the grana which bear the chlorophylls on their closely packed lamellae (shown as C on figure 4). Both the

cytochromes are also found to be entirely confined to the grana (figure 6). This close propinquity of the cytochromes to the chlorophylls (perhaps in a single monolayer) would allow high efficiency in any electron transport cycle in which they were common partners.

Extracted mitochondria are among the particles precipitated from a tissue homogenate by centrifugation for 20–30 minutes at between 5000 and 20 000 *g*. It is now sufficiently clear that the fraction is not homogeneous; but attempts at further fractionation have not yet led to definite results. The true mitochondria are taken to be those particles with a definite external membrane having infoldings of various forms into a central cavity occupied by a fluid sap (figure 4, E).

They can now perhaps be considered the sites of respiratory oxidations just as definitely as chloroplasts have long been recognized as the sites of photosynthetic reductions. The respiratory cytochromes are localized in them; visual spectroscopic observation of these cytochromes in organs such as wheat and barley roots is difficult on account of the dilution, but, when the mitochondria are extracted and the rest of the cell homogenate discarded, the characteristic bands of cytochromes a, b, and c are easily seen. Such observations, both visual and spectrophotometric, have now been made with a variety of species. The mitochondria of *Arum* spadices contain also cytochrome b_7, and the typical deformation of the absorption curve obtained with a suspension of wheat mitochondria suggests the presence of cytochrome c_1 [14].

The mitochondrial cytochromes appear to be attached to the walls. When the mitochondria are disintegrated ultrasonically and the sap is discarded with the suspension medium, the cytochromes a and b remain attached to the solid particles. Cytochrome c also remains at least in part. Owing to its solubility, it is not certain that all the cytochrome c is naturally attached to the mitochondrial membranes.

It has been reported that not all the cytochrome content of a plant cell is precipitated with the chloroplasts and mitochondria, and that some comes down with the 'microsome' fraction precipitated by prolonged centrifugation at 50 000 –100 000 *g*. The few results available are, however, somewhat conflicting as to the nature of the cytochromes concerned.

No cytochromes have yet been recognized in either animal or plant nuclei, although nuclei free from mitochondria can be obtained from viable wheat embryos. In the authors' laboratory these have shown no spectroscopically detectable cytochromes in concentrations twenty times (with respect to total nitrogen) that required with mitochondria. No cytochrome oxidase activity can be demonstrated manometrically.

Although haem compounds occur in the 'soluble fraction' of cytoplasm none of them have yet been shown to be cytochromes, and no cytochromes are attached to the cell wall.

Of those definitely known to occur in plant tissues cytochrome b_3 is the most uncertain in its distribution and its functions. The rest fall into two well defined groups, those present in chloroplasts and probably concerned with electron transport in photosynthesis; and those present in mitochondria and probably concerned with electron transport in respiration.

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Book reviews

THERMODYNAMICS

Thermodynamics and Statistical Thermodynamics, *by J. G. Aston and J. J. Fritz. Pp. xiv+556. John Wiley & Sons Inc., New York; Chapman and Hall Ltd, London.* 1959. 66*s. net.*

Like many other books on thermodynamics, this text originates in a lecture course; it is based on material presented to a one-year graduate course at the Pennsylvania State University, but would probably be more useful, in the United Kingdom, to honours students. One of the primary considerations in teaching thermodynamics is to instil confidence in using its results. In this work, this is well provided for by four hundred problems and exercises to be worked out in conjunction with very useful tables collected in appendices to the text. Many of the chapters deal with material which is by now available in several alternative textbooks, but the treatment of energy levels of polyatomic molecules, and of their thermodynamic functions, seems particularly valuable for teaching purposes, since it is not readily found in the same clear form elsewhere. Chapters on the solid state deal with the principal elementary theories, but do not give many experimental illustrations. The book will prove useful to those planning lecture courses in thermodynamics, and some of the chapters should make useful alternative reading, at a fairly elementary level, for honours students.

A. R. UBBELOHDE

NUCLEAR PHYSICS

Proceedings of the International Conference on Nuclear Physics, Paris, 7–12 July 1958, *presented by P. Gugenburger. Pp. xxiv+950. Crosby Lockwood & Son Ltd, London.* 1959. 140*s. net.*

The value of permanent records of the proceedings of a scientific conference may be questioned. Much depends on what happened at the conference, and judicious editing is required to produce a uniform book.

The present volume on low-energy nuclear physics is partly complementary to the standard works on the subject. It contains useful summaries, presented by international authorities, on such topics as neutron, proton, and electron scattering by nuclei, on direct interactions, heavy-ion reactions, photonuclear reactions, weak interactions, nuclear structure models, and on progress in the basic theory of nuclear properties.

The Paris conference abstained from presenting short communications of the kind which have depressed other recent conferences; instead, summaries of short papers submitted to the conference were included in the principal talks and led to more coherent and interesting discussion. This book consequently gives a fairly up to date and lively picture of the main problems, lines of progress, and points of controversy.

The main fault is the indiscriminate inclusion of 150 short communications. These occupy two-thirds of the book. Most, if not all, would normally appear in the standard literature of nuclear physics. It would have been better to include only those papers which were basic to the arguments of the principal speakers.

J. E. LYNN

PLASMA PHYSICS

Plasma Physics and the Problem of Controlled Thermonuclear Reactions, *Vol. III. Edited by M. A. Leontovich (translated from the Russian by J. B. Sykes). Pp. 422. Pergamon Press Ltd, London.* 1959. £8 net.

This volume is the third of a four-volume collection of papers on plasma physics describing hitherto unpublished work done by Russian physicists in the years 1954–57. It contains twenty-six papers, falling roughly into three groups: theoretical papers on the fundamental problems, theoretical papers describing or proposing controlled thermonuclear devices, and papers giving the results of experiments on idealized versions of such devices. On fundamental problems, there are papers deriving transport equations and distribution functions in the presence of strong fields; other papers deal with the hydrodynamic equations governing plasma motion. There are several papers investigating energy loss at high temperatures and the stability of plasmas confined by magnetic walls. Experimental papers discuss current and field intensities and the instruments used in measuring them. This is an interesting collection giving some idea of the extent and duration of the Russian programme in this field, and the Pergamon Press has done a valuable service in making them available in English, albeit at a price which makes it prohibitive for private purchase. One hopes in future, however, that all the Russian work in this field will appear in the journals in the normal way.

S. F. EDWARDS

TERPENOIDS

The Chemistry of Natural Products, *edited by K. W. Bentley.* Vol. II, Mono- and Sesquiterpenoids, *by P. de Mayo. Pp. vii+320.* 52*s. net.* Vol. III, The Higher Terpenoids, *by P. de Mayo. Pp. vii+239.* 42*s. net. Interscience Publishers Inc., New York; Interscience Publishers Ltd, London.* 1959.

From the beginnings of natural-product chemistry, organic chemists have been interested in the terpenoids. These are a fascinating group of compounds and, as the author states in his preface, 'correctly studied, present almost a microcosm of organic chemistry'.

In recent years very rapid progress has been made, especially with the sesquiterpenoids, diterpenoids, and triterpenoids, and there has been a growing need for an up to date and critical general account of their chemistry. These two volumes admirably fill this need and will be of use to all those interested in natural products (and reaction mechanisms). As their sizes indicate, the two volumes are not reference books.

Volume II, besides covering the chemistry of monoterpenoids and sesquiterpenoids, also provides a useful survey of the applications of infrared, ultraviolet, and nuclear magnetic resonance spectroscopy and of molecular rotation data. Accounts of conformational analysis, Wagner-Meerwein rearrangements, and of the themes of classical and non-classical carbonium-ion theories are also to be found.

Volume III covers diterpenoids and triterpenoids, and in addition provides a review of the biosynthesis of terpenoids. So many advances have been made in the field of biosynthesis since the volume went to press that this picture is, unfortunately, already incomplete.

T. G. HALSALL

STEROIDS

Steroids, *by L. F. Fieser and M. Fieser. Pp. xvii+945. Reinhold Publishing Corporation, New York; Chapman and Hall Ltd, London.* 1959. £7 4*s. net.*

During the past ten years, total chemical syntheses of cholesterol, of progestational, androgenic, and adrenal hormones, and of Vitamin D_3 have been attained. The absolute configurations of steroids were deduced, and the influence of conformation on reactivity was explained. The pathway of biogenesis of steroids was discerned and their classification as polyisoprenoids established. Microbiological oxidations and a bewildering variety of partial syntheses provided a range of artificial hormones surpassing in certain activities their natural prototypes. The complex structures of several steroid alkaloids were determined.

The same ten years separate the present version of 'Fieser' from its precursor, and it is a pleasure to remark that the authors have done even better this time. Their approach might be described, after the manner of Polonius, as chemical-biological-historical-personal; it combines an enveloping interest in the academic and practical aspects of the subject with an awareness of its history, and of its status as a piece of human effort. This outlook, combined with much literary skill, has produced a work both authoritative and entertaining. Advice from many specialists has contributed to accuracy without destroying unity of style; only one error of interpretation was noticed. The book can be recommended to any organic chemist.

J. W. CORNFORTH

CELLULOSE

Recent Advances in the Chemistry of Cellulose and Starch, *edited by J. Honeyman. Pp. viii+358. Heywood & Co. Ltd, London.* 1959. 60*s. net.*

This book, which is largely concerned with the chemistry of cellulose, consists of twelve specialist chapters written by experts in their various fields. The chapters were in each case based on lectures delivered at the Manchester College of Science and Technology. They are all highly condensed, and indeed some tend to read like lectures, especially chapter 1, 'Introduction to the Chemistry of Carbohydrates'. Here the whole of monosaccharide chemistry, including conformational analysis, is condensed into about twenty pages, with another twenty pages of formulae. It is somewhat disconcerting to have to find all these formulae (probably from the slides illustrating the lecture) at the end of the chapter. The account is valuable, however, because some sections, such as conformational analysis and periodic oxidations, have not been presented so well elsewhere.

The third and fourth chapters on the hydrolytic and oxidation degradation of cellulose and on the alkaline degradation of cellulose contain a lot of new material and are most timely and valuable. They will be of particular use to the textile chemist.

For the physical chemist in particular, the chapters on the crystal structure, fine structure, and mechanical properties of cellulose, absorption of water by cellulose and starch, and degree of polymerization of both polysaccharides provide excellent summaries of present knowledge and give valuable references. For the industrial and textile chemist, chapters on cellulose derivatives, and on the preparation, properties, and uses of starch will be an added attraction, while an important final chapter on enzymic synthesis and degradation nicely rounds off the subject of starch and cellulose.

The book is attractively presented, it contains many well-drawn diagrams and tables, and a few good plates. The references are well chosen, and author and subject indices are provided

Although the book is intended for postgraduates, advanced students will find it readable and useful. The editor is to be congratulated on presenting such a well-balanced and informative course of carbohydrate lectures, and in arranging for their publication in this excellent manner.

M. STACEY

CRYSTALLOGRAPHY

Vector Space and its Application in Crystal-Structure Investigation *by M. J. Buerger. Pp. xiv+347. John Wiley & Sons Inc., New York; Chapman and Hall Ltd, London.* 1959. 96*s. net.*

The results of experiments in X-ray crystallography provide incomplete data for determining the crystal structure, and this leads to the phase problem. The methods of vector space have been developed to assist in the solution of this problem, and both the power and the limitations of the methods are made clear in this book. The first half covers Patterson functions, maps and projections, and Harker sections and implication theory. Vector space is introduced, and its properties are developed. There are also chapters on image-seeking functions, and their uses in projections and in three dimensions.

The book is intended for the research worker, but only an elementary knowledge of crystal-structure analysis is assumed. On the other hand, while the subject matter is fairly mathematical, no special mathematical background is required, except the capacity to handle algebraic and analytical expressions and to grasp geometrical concepts in space. The work is very well illustrated and the style is not condensed. There are plenty of illustrative examples, and adequate references to the literature. A good deal of the material has been published previously by the author in various journals, but the unified presentation now at hand should make it easier for new readers to learn about the methods and applications of vector space.

L. S. GODDARD

CRYSTAL GROWTH

Dendritic Crystallization, *by D. D. Saratovkin. Pp.* 126. *Consultants Bureau Inc., New York; Chapman and Hall Ltd, London.* 1959. $6.

This book is directed to metalworking engineers, but contains some material concerning crystal growth generally.

The author uses a binocular microscope to observe dendrites, such as those of ammonium chloride grown from aqueous solutions. Some photomicrographs and a few line drawings are reproduced as stereoscopic pairs; it seems an unnecessary complication to assume that the reader will not have a stereoscope.

The growth forms of real crystals are reviewed and related to dendritic and skeletal forms. The effects on crystal growth of surface-active impurities are referred to in some detail, and the conclusion is drawn that the explanation of dendritic growth is to be sought in the impurities which are always present. The last forty pages are devoted to eutectics, and to dendritic structures in alloys.

H. M. POWELL

OCEANOGRAPHY

Oceanography and Marine Biology, *by H. Barnes. George Allen and Unwin Ltd, London.* 1959. 35*s. net.*

The author has brought together a lot of information useful for beginners rather than advanced students. He shows how knowledge of the sea is obtained, and describes sufficient problems and results to encourage people

to start making observations, and to put forward their own ideas. It deals with sampling of the water, plants, animals, and sediments, with practical comments on the limitations and advantages of different methods. It touches on some of the problems that arise in analysing the collected material.

The sea is not very transparent to light waves, but short-range photographs of the sea floor are very useful, and a range of underwater cameras is described. The possibilities of aerial photography are discussed. The author gives an account of his pioneer work on the application of underwater television to biological problems. In a good chapter on echo-sounding, which emphasizes the value of acoustic techniques in the study of the sea, there is a rather misleading indication that the frequency of sound emitted by a magnetostriction oscillator can be varied by varying the frequency with which the current is changed. In practice, the oscillator must be driven at its resonant frequency, which is determined by the physical dimensions.

A few more references in the text showing whence the information was derived and where more detail can be found would have been useful.

G. E. R. DEACON

MOLECULAR BIOLOGY

A Symposium on Molecular Biology, *edited by Raymond E. Zirkle. Pp. viii+ 348. University of Chicago Press, Chicago; Cambridge University Press, London.* 1959. 56*s.* 6*d. net.*

Molecular biology has gained much prestige in recent years, and organizers of conferences and editors of conference volumes have been eager to help in the development of the subject. Many research workers in the field—and the reviewer is one of these—consider that there are too many conferences on the same subjects, and that writing articles for conference volumes (often a condition for speaking at the conference) impedes research and its adequate publication in journals. On the other hand, most workers agree that conferences can be very rewarding if they are carefully and critically planned, have a clear purpose, and are aimed at benefiting some definite category of scientist, and if the conference volumes are published efficiently.

These remarks provide a basis for criticism of the book. The separate chapters originated as lectures in a seminar series and a symposium held at the University of Chicago between November 1956 and March 1957 under the auspices of the Chicago-Frankfurt Inter-University Program. Judging from the book, it seems that both general audience and specialists should have derived great benefit from the lectures at Chicago. Most of the authors are leading authorities in rapidly developing fields. It is more difficult, however, to decide how worth while it was to publish the lectures. The volume is worth having in any comprehensive biological library, and is of value to the general reader because it is unique in covering a wide range of topics which are well representative of molecular biology. If it had been published more promptly it would also have been of use to the specialist reader. Many of the chapters were excellent when they were written. The discussion of the role of ribonucleic acid in protein synthesis is still very valuable, but the authoritative articles on structure of DNA and on synthesis of DNA are, unfortunately, out of date, Progress is so rapid in this field that unless lectures go from lecture hall to printing press in no more than a few months, their publication becomes of doubtful value. This is illustrated by the note referring to microsomal particles and added by the author in proof at the end of his chapter: 'Knowledge of the particles has advanced so rapidly that reading this paper is like visiting a museum.' It is a pity that the value of this volume has been so greatly reduced because of late publication.

M. H. F. WILKINS

MAGNESIUM

The Physical Metallurgy of Magnesium and its Alloys, *by G. V. Raynor. Pp. ix+ 531. Pergamon Press Ltd, London.* 1959. 75*s. net.*

Professor Raynor has set out to develop the complete metallurgy of a metal in terms of basic physical concepts. At the outset he warns those readers looking for up to date details of magnesium technology to seek them elsewhere, since he only introduces them where they assist the theoretical treatment. The book can be considered as Professor Raynor's profession of faith, and he hints that it was as much magnesium's strategic position in the Periodic Table as its attractive properties as an engineering material that caused him to choose it for this exercise in the basic theoretical treatment of a metal. Magnesium is particularly valuable for the author's purpose because it is one stage more complex than the simple monovalent elements and therefore more of a test of recent progress in physical metallurgical theory. On the other hand, it is not so complex as, for instance, the Group IVa metals, which so far appear to have resisted a similar theoretical approach.

After an introductory chapter and a compact review of the physical properties of unalloyed magnesium, there are five chapters on the general physical metallurgy of the metal and its alloys, namely, electronic structure of solid magnesium, theoretical factors underlying the deviations in lattice spacing of magnesium alloys from Vegard's law, alloying behaviour of magnesium, crystal structures of intermediate alloy phases type by type, and deformation behaviour. The remaining nine chapters discuss in detail, according to the Periodic Table, available information on alloys of magnesium, and include chapters on miscellaneous metals, systems formed with gaseous elements and compounds, and a final chapter summarizing the influence of alloying on the mechanical properties of magnesium.

The physical metallurgist will find the book a fascinating essay in the use of theoretical methods to explain known behaviour of a relative newcomer among industrial metals. It is to be hoped that, in the fullness of time, Professor Raynor will perform a similar service for some of the newer and even more complex metals.

J. W. RODGERS

ZINC

Zinc: the Science and Technology of the Metal, its Alloys and Compounds, *edited by C. H. Mathewson (in co-operation with the American Zinc Institute). Pp. xii+ 721. Reinhold Publishing Corporation, New York; Chapman and Hall Ltd, London.* 1959. £7 16*s. net.*

Dr C. H. Mathewson and his very able associates are to be congratulated on this excellent work which offers, for the first time, a really comprehensive treatise on the metallurgy of zinc. Although much has been published in the technical press on this subject, never before have forty-five leading specialists joined forces to produce a single volume containing such a wealth of information.

The fourteen chapters, besides dealing very thoroughly with the basic technology of zinc, cover such topics as

history, economics, geology, ore treatment, refining, processing, alloy technology, the use of zinc for the extraction of other metals, zinc compounds, the biological significance of zinc, and its use in agriculture. This breadth of treatment, and the great number of references cited by the authors of the various chapters, make this book essential to all those who are interested in the technology of zinc and its uses, whether actual or potential, in industry.

Any criticism that can be levelled at the book can only be on the grounds of what has been omitted rather than what has been said. The writer regrets that some of the unsuccessful or only partly successful developments of the past have not received attention; these often provide the inspiration for later success. But here one must sympathize with the editor and his associates in their efforts to keep this volume to a manageable size. L. J. DERHAM

HISTORY OF SCIENCE

Science and Civilization in China, *Vol. III*, Mathematics and the Sciences of the Heavens and the Earth, *by J. Needham (with the collaboration of L. Wang). Pp. xlvii+877. Cambridge University Press, London.* 1959. £7 10*s. net.*

This is a highly important work, for those who read Chinese and are also familiar with the sciences are rare.

The volume includes studies of mathematics, astronomy, geography, and seismology, in each of which the Chinese made important discoveries—most of all in mathematics, for which the Chinese have shown, and still show, unusual ability. Already, in the pre-Christian era, before the appearance of our Arabic or Hindu numerals, the Chinese were using place-value in writing numbers. For the purpose of calculation, numbers were written from left to right, as we do, although in documents they were written like other sentences, in columns from top to bottom and right to left. There were names for each place-value, as 'tens', 'hundreds', 'ten-thousands', which were omitted in calculation. The number zero is recorded first as a dot or as a circle in seventh-century Indonesian writings. Earlier it was expressed by leaving a place vacant in a number. My guess is that this zero originated from the circle still used in certain Chinese books to denote the end of a paragraph or section, instead of the blank space otherwise used for that purpose. Place-value is, then, a Chinese discovery, zero possibly also. Fourteen Chinese 'firsts' in mathematics are listed (page 146).

In astronomy the Chinese likewise had firsts. In the second century A.D. they were said to have listed 14 000 stars. The *Almagest* has only 1028. Europe of the Middle Ages had no star charts, but there is a still extant tenth-century Chinese star chart.

This book opens up a vast field and summarizes an encyclopaedic research. The huge bibliography of 118 pages is daunting. Yet one wonders if the author can have critized all his sources adequately. For example, he accepts Tung Tso-pin's conclusions concerning China in the second millennium B.C. without realizing that this very eminent paleographer is quite naive regarding astronomy and calendrical science, so that his reconstruction of the Yin calendar and dates is based on erroneous assumptions. Not even an encyclopaedist can examine in detail each original document. Here the author appears to have depended upon unduly nationalistic Chinese advisers.

H. H. DUBS

HISTORY OF MATHEMATICS

Mathématiques et mathématiciens, *by P. Dedron and J. Itard. Pp. iv+433. Magnard, Paris.* 1959. *N. Fcs* 19.80.

This book sketches the growth of pure mathematics from its earliest beginnings to about the end of the eighteenth century. Two-thirds of its length is occupied by the historical narrative, built up round the achievements of the great 'legislators' of the subject. The concluding section affords scope for a more methodical discussion of special topics: these include the historic systems of numeration and notation, the equation of the second degree, and the three classic problems of Greek geometry. The text is interwoven with synopses of the chief mathematical classics and with short illustrative extracts from them.

There is an understandable preference for French mathematicians, the seventeenth century being represented by Fermat, Roberval, Descartes, Desargues, and Pascal, with only brief mention of others; but Newton and Leibniz are assigned their due place among the pioneers of eighteenth-century classical analysis.

Technically, the book falls within the limits of higher school studies, and it could help to enlarge the horizon of the student. It should also provide a suggestive syllabus for an historical approach to the teaching of mathematics. It is well provided with portraits, specimen pages and 'period' illustrations, as well as with numerous geometrical diagrams.

A. ARMITAGE

SULPHUR

Brimstone: The Stone that Burns, *by W. Haynes. Pp. xix+308. D. Van Nostrand Co. Ltd, London.* 1959. 45*s. net.*

This book is based on the 1942 edition. This has been condensed, the wartime story completed, and post-war developments (including recovery from 'sour' natural gas) told mainly in new chapters. Herman Frasch (1851–1914) left his native Germany for America in 1871, became a specialist on petroleum, and set up as a consulting chemist at Cleveland. Later, he became the Standard Oil Company's first director of research. Having patented desulphurization processes for paraffin wax and paraffin, he then perfected a method for extracting sulphur from the limestone caps of salt-domes associated with oilfields. Unlimited water and cheap fuel were essential, as the sulphur was melted *in situ* and pumped out. Soon, however, pumps were replaced by compressed air. Extracted and refined in one process, Frasch sulphur competed strongly with Sicilian mined sulphur. Exploitation of deposits around the Gulf of Mexico followed—in Louisiana, Texas, and Vera Cruz. Hot-water mining under marsh, lake, and sea provides an absorbing story of enterprise, perseverance, and endurance. The book is copiously illustrated, documented, and supplied with statistics covering production and uses of sulphur. F. W. GIBBS

Short notices of books

(These notices are descriptive rather than critical and are designed to give a general indication of the nature and scope of the books.)

Scientific Russian, *by G. E. Condoyannis. Pp. xii+225. John Wiley & Sons Inc., New York; Chapman and Hall Ltd, London.* 1959. 28*s. net.*

The aim of this book is to provide a background of grammar such that it can be used, in conjunction with a dictionary, in the translation of Russian technical and scientific writings. It is written with the non-linguist in mind, and for this reason carefully explains the grammatical terms that are used.

The Structure and Evolution of the Universe, *by G. J. Whitrow. Pp.* 212. *Hutchinson & Co. (Publishers) Ltd, London.* 1959. 21*s. net.*

Although this was intended to be a second edition of the author's 'The Structure of the Universe', published in 1949, so many advances have been made in astronomy and cosmology that the book has had to be almost completely rewritten. The new material includes consideration of data from radio-astronomy and of observations using the Palomar telescope, and, on the theoretical side, of modern ideas about the problems of the development of the universe.

Advances in Spectroscopy, *Vol. I, edited by H. W. Thompson. Pp. ix+363. Interscience Publishers Inc., New York; Interscience Publishers Ltd, London.* 1959. 85*s. net.*

It is intended that this series will cover all aspects of spectroscopy, so that workers in particular fields may know what progress is being made elsewhere. This volume includes chapters on the spectra of polyatomic free radicals, on spectroscopy in the vacuum ultraviolet, and on high-resolution Raman spectroscopy, and two chapters on infra-red spectroscopy.

Some Problems in Chemical Kinetics, *Vol. II, by N. N. Semenov (translated by M. Boudart). Pp. vii+331. Princeton University Press; Oxford University Press, London.* 1959. 36*s. net.*

The subjects covered by this volume include competition between chain reactions and reactions between saturated molecules; there are a number of chapters under the general heading of branched chain reactions and thermal explosions. Appendices cover additions to Vol. I, the method of the activated complex, and quantum-mechanical calculations of the activation energy.

Isotopic Tracers, *2nd edition, by G. E. Francis, W. Mulligan, and A. Wormall. Pp. xxi+524. The Athlone Press, London.* 1959. 52*s.* 6*d. net.*

The advances made since 1954 in the principles and applications of isotopic tracers have necessitated an extensive revision and expansion of the first edition of this book. Particular attention is given to recent work in instrumentation and in the use of scintillation and gas-counting techniques, and new sections have been added on Vitamin B_{12}, on photosynthesis, on radioactivation analysis, and on 'self-radiation' labelling with tritium. There is also a new chapter on the study of the kinetics of biological processes, and the section on hazards and precautions has been brought up to date.

Lehrbuch der organischen Chemie, *by P. Karrer (thirteenth edition, revised and enlarged). Pp. xx+1057. Georg Thieme Verlag, Stuttgart.* 1959. *DM.* 60.

This textbook is too well known to teachers and students of chemistry to need introduction. That it now appears in its thirteenth edition since it was first published in 1927 and has been translated into six foreign languages is a measure of its wide acceptance. In preparing the new edition substantial revisions have been made, many of them in collaboration with distinguished authorities on the subjects in question.

Advances in Enzymology, *Vol. XXI, edited by F. F. Nord. Pp. v+521. Interscience Publishers Inc., New York; Interscience Publishers Ltd, London.* 1959. $12.50.

There are eight reviews in this book; the subjects covered include mitochondrial metabolism, the mechanism of metal ion activation of enzymes, enzymic reactions in the synthesis of purines, the biosynthesis and function of carotenoid pigments, and the enzymic synthesis of pyrimidines. The volume also contains cumulative indexes of Vols. I to XXI.

British Medical Bulletin, *Vol. XV, No.*3. Current Virus Research, *edited by C. H. Andrewes. Pp.* 175–250. *The British Council, London.* 1959. 20*s. net.*

The last survey of virology in this bulletin was published in 1953; recent progress has been most striking in somewhat different fields than those reported then. This review includes papers on virus interference and interferon, recent advances in the virology of trachoma, inclusion conjunctivitis and allied diseases, enteroviruses, and measles. Two studies in the field of epidemiology are of influenza and its complications, and of myxomatosis.

Immunopathology. The First International Symposium, *edited by P. Grabar and P. Miescher. Pp. xv+520. Benno Schwabe & Co., Basle.* 1959. 65 *Sw. Fcs.*

This symposium was held at Basle in 1958, and the papers that were presented are printed here in their original language, English, French, or German, together with a concise version of all important contributions to the discussion. The two main themes of the meeting were the basic immunological phenomena from the point of view of inflammatory reactions, and the more important clinical applications of immunopathology. Forty-three papers were delivered.

Masers, *by G. Troup. Pp. x+108. Methuen & Co. Ltd., London; John Wiley & Sons Inc., New York.* 1959. 13*s.* 6*d.* net.

The study of devices used to obtain Microwave Amplification and Oscillation by Stimulated Emission and Radiation, known as Masers, is a new one, and this book is one of the first to be published on it. It outlines the stimulated emission process and the methods used to obtain the conditions necessary for amplification, and includes a section on the effect of physical processes on amplifier efficiency, and discussions of both the stimulated emission and the resonant-cavity amplifiers.

Perfumes, Cosmetics and Soaps, *Vol. II, 7th edition, by W. A. Poucher. Pp. xii+453. Chapman and Hall Ltd, London.* 1959. 75*s. net.*

In this edition the author uses a new classification of odours, based on the volatility of a wide range of aromatic substances. The volume is devoted to perfumes of all types, and includes an outline of the history of perfumes, together with chapters on the production of natural perfumes, on the purchase and use of flower absolutes, and an extensive collection of recipes for particular perfumes.

Notes on contributors

M. B. GOTTLIEB,
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Was born in Chicago, Illinois, in 1917 and educated at the University of Chicago. He was a research associate in Harvard University from 1943 to 1945; an instructor in physical sciences at the University of Chicago from 1945 to 1946; and a research assistant in physics from 1946 to 1950. From 1950 to 1954 he was Assistant Professor of Physics in the State University of Iowa, where his research interests were in the field of cosmic rays. Since 1954 he has been at Princeton University, where he is now Associate Director of Project Matterhorn.

E. N. DA C. ANDRADE,
D.Sc., Ph.D., LL.D., F.R.S.,

Was born in London in 1887 and was educated at St Dunstan's College, the Universities of London, Manchester, and Heidelberg, and the Cavendish Laboratory, Cambridge. He served as an artillery officer in France in the war of 1914–18, and afterwards was for some years professor at the Artillery College (later the Royal Military College of Science). In 1928 he was appointed Quain professor of physics in the University of London and established a flourishing school of physics, known for fundamental work on the mechanical properties of the solid and liquid state. In 1950 he was appointed Director in the Royal Institution of Great Britain and Director of the Davy Faraday Research Laboratory, but in 1952 resigned, like his predecessor. He maintains close connection with colleagues overseas, and is *Membre Correspondant de l'Académie des Sciences, Institut de France* and *Membre d'Honneur de la Société Française de Physique.*

D. C. MARTIN,
C.B.E., B.Sc., Ph.D., F.R.I.C., F.R.S.E.,

Was born in 1914 and educated at the University of Edinburgh, where he did research in physical chemistry. During the war he was in the Directorate of Scientific Research in the Ministry of Supply, and among other duties was Secretary of the Explosives Research Committee (Chemistry). From 1945 to 1946 he was General Secretary of the Chemical Society, and he has been Assistant Secretary of the Royal Society since 1947.

M. C. SHELESNYAK,
B.A., Ph.D.,

Was born in 1909 in Chicago and was educated at the University of Wisconsin, and Columbia University, New York, and subsequently became a Research Associate at Mount Sinai Hospital and at Beth Israel Hospital, both in New York. He served in the United States Naval Reserve and then as Head of the section of the Office of Naval Research devoted to the study of environmental physiology and human ecology. From 1949 to 1950 he was a lecturer in geography at Johns Hopkins University. In 1950 he became Professor of Endocrine and Reproduction Physiology at the Weizmann Institute of Science, Israel. His interests in research are in reproduction physiology, the physiology of adolescence, and, parcularly, in the mechanism of nidation.

H. C. UREY,
B.S., Ph.D., D.Sc., For. Mem. R.S.,

Was born in Walkerton, Indiana, in 1893 and educated at Montana State University and the University of California. He was Associate Professor of Chemistry at Columbia University from 1929 to 1934, Professor from 1934 to 1945, and Professor of Chemistry at the Institute of Nuclear Studies, Chicago, from 1945 to 1952. From 1952 to 1958 he was Ryerson Distinguished Service Professor of Chemistry there; at present he is Professor-at-Large of Chemistry in the University of California. He has published on absorption spectra and the structure of molecules, the discovery of deuterium, the properties and separation of isotopes, and chemical problems of the origin of the Earth. He was awarded the Nobel Prize for chemistry in 1934.

A. R. LONGWELL,
B.A., Ph.D.,

Was born in Buffalo, New York, and educated at Southwest Missouri State College and the University of Missouri. From 1957 to 1959 she was a resident research associate at the Argonne National Laboratory, Lemont, Illinois. At present she is at the Genetics Institute, Lund, Sweden; this year she will return to the Children's Cancer Research Foundation, Harvard Medical School, Boston, Massachusetts. Her research is concerned with the cytology and cytochemistry of aneuploidy in plants and in tumours, with emphasis on the nucleolus-producing activity of particular chromosome types.

M. MOTA,
Engº Agrónomo

Was born in Lisbon in 1922 and educated at the University of Lisbon. He was head of the Cytogenetics Department of the Plant Breeding Station at Elvas (Portugal) from 1948 to 1955. Since then he has been at the National Agricultural Station, Sacavém. From 1957 to 1959 he worked at the Oak Ridge National Laboratory, Tennessee, and at the Argonne National Laboratory, Illinois. His principal interests are in the artificial production of polyploids and amphiploids in agricultural plants, and in problems of chromosome morphology, ultrastructure, and dynamics.

W. O. JAMES,
M.A., D.Phil., F.R.S.,

Was born in 1900 and was educated at Tottenham Grammar School and the Universities of Reading and Cambridge. He worked first at Rothamsted on the potassium nutrition of plants and was successively Demonstrator and Reader in Botany in the University of Oxford, and from 1927 led a school of teaching and research in plant physiology, especially concerned with nutrition and respiration. He is now Professor of Botany, and Head of the Department of Botany and Plant Technology at Imperial College, London. He is an editor of the 'New Phytologist', and has published 'The Biology of Flowers' (with A. R. Clapham), 'Background to Gardening', 'Plant Respiration', some textbooks, and numerous papers in several branches of botany.

RACHEL M. LEECH,
B.A.,

Was born in 1936 and was educated at Prince Henry's Grammar School, Otley, and read botany at St. Hilda's College, Oxford, where she was Christopher Welch and Naples Biological Scholar; she worked from 1957 to 1958 at the *Stazione Zoologica*, Naples. Since then she has been studying plant metabolic physiology in Oxford and at Imperial College, London.

Some books received

(*Note.* Mention of a book on this page does not preclude subsequent review.)

GENERAL SCIENCE

Paul Ehrenfest Collected Scientific Papers, *edited by Martin J. Klein. Pp. xii* +632. *North-Holland Publishing Co., Amsterdam.* 1959. 90*s. net.*

La menace radioactive, *by A. Pirie. Pp. xx*+128. *Dunod, Paris.* 1959. *Fcs.* 950.

Science in Industry—Policy for Progress, *by C. F. Carter and B. R. Williams. Pp. ix*+186. *Oxford University Press, London.* 1959. 21*s. net.*

Soviet Space Science, *by Ari Shternfeld. Pp. vii*+361. *Hutchinson & Co. (Publishers) Ltd, London.* 1959. 30*s. net.*

PHYSICS

Atomic Age Physics, *by Henry Semat and Harvey E. White. Pp. vii*+230. *Rinehart and Co. Inc., New York; Chapman and Hall Ltd, London.* 1959. 16*s. net.*

CHEMISTRY

Annual Reports on the Progress of Chemistry, *Vol.* LV. 1958. *Pp. vi*+527. *The Chemical Society, London.* 1959. £2 *net.*

Catalysis, Vol. VI, Hydrocarbon Catalysis, *edited by Paul H. Emmett. Pp. vi*+706. *Reinhold Publishing Corporation, New York; Chapman and Hall Ltd, London.* 1958. 156*s. net.*

The Chemical Elements, *by Helen Miles Davis (revisions by Glenn T. Seaborg). Pp.* 204. *Science Service Inc., Washington; Ballantine Books Inc., New York.* 1959. 50 *cents.*

Chemistry for Engineers, an Introductory Course, *by E. Cartmell. Pp. vii*+172. *Butterworths Scientific Publications, London.* 1959. 25*s. net.*

Organic Syntheses, *Vol.* XXXVIII, *edited by J. Sheehan. Pp. vii*+120. *John Wiley & Sons Inc., New York; Chapman and Hall Ltd, London.* 1959. 38*s. net.*

Review of Benzole Technology 1958. *Pp.* 145. *The National Benzole and Allied Products Association, London.* 1959. 22*s.* 6*d. net.*

GEOLOGY

Application de la géologie aux travaux de l'ingénieur, *by Jean Goguel. Pp.* 358. *Masson et Cie., Paris.* 1959. *Fcs.* 4300.

BIOLOGY

Biogeography and Ecology in Australia, *edited by A. Keast, R. L. Crocker, and C. S. Christian. Pp.* 640. *Dr W. Junk, The Hague.* 1959. 65 *Guilders.*

Darwin's Biological Work: Some Aspects Reconsidered, *edited by P. R. Bell. Pp. xiii*+343. *Cambridge University Press, London.* 1959. 40*s. net.*

Darwin's Place in History, *by C. D. Darlington. Pp. ix*+101. *Basil Blackwell, Oxford.* 1959. 9*s.* 6*d. net.*

Field Studies, *Vol.* I, *No.* I, *May* 1959. *edited by A. R. Clapham, D. L. Dineley, T. R. E. Southwood, G. E. Hutchings, J. H. Barrett, and F. J. Bingley. Pp.* 142. *Field Studies Council, London.* 1959. 5*s. net.*

La sexualité des bactéries, *by E. L. Wollman and F. Jacob. Pp.* 248. *Masson et Cie., Paris.* 1959. *Fcs.* 3000.

BOTANY

Algae in Water Supplies, *by C. Mervin Palmer. Pp. viii*+88. *U.S. Government Printing Office, Washington.* 1959. $1.

ZOOLOGY

Reproduction and Infertility. III Symposium, *edited by F. X. Gassner. Pp.* 273. *Pergamon Press Ltd, London.* 1958. 42*s. net.*

Mission Robert Ph. Dollfus en Egypte, *by R. Ph. Dollfus. Pp.* 285. *Centre National de la Recherche Scientifique, Paris.* 1959. *Fcs.* 4500.

Die tertiäre Pflanzen- und Tierwelt der Braunkohle des Geiseltales, *by G. Krumbiegel. Pp.* 156. *A. Ziemsen Verlag, Wittenberg Lutherstadt.* 1959. *DM.* 5.20.

MEDICINE

Les bases de l'alimentation, *by Raymond Ferrando. Pp. viii*+246. *Vigot Frères, Paris.* 1959. *N. Fcs.* 29.50.

British Veterinary Codex 1953 (Supplement 1959). *Pp. xviii*+134. *The Pharmaceutical Press, London.* 1959. 35*s. net.*

Documenta Ophthalmologica, *Vol.* XIII, *edited by G. von Bahr et al. Pp. xii*+516. *Dr W. Junk, The Hague.* 1959. 75 *Guilders.*

ENGINEERING

The Electric Arc, *by J. M. Somerville. Pp. ix*+150. *Methuen & Co. Ltd, London; John Wiley & Sons Inc., New York.* 1959. 12*s.* 6*d. net.*

History of the Gear-cutting Machine, *by Robert S. Woodbury. Pp. iv*+135. *The Technology Press, Massachusetts Institute of Technology, Cambridge, Mass.* 1958. $3.

History of the Grinding Machine, *by Robert S. Woodbury. Pp.* 191. *The Technology Press, Massachusetts Institute of Technology, Cambridge, Mass.* 1959. $3.50.

Selected Topics on Ballistics, *edited by Wilbur C. Nelson. Pp. vii*+280. *Pergamon Press Ltd, London.* 1959. 63*s. net.*

AGRICULTURE

Bananas, *by N. W. Simmonds. Pp. xvi* +466. *Longmans, Green & Co. Ltd, London.* 1959. 45*s. net.*

A Hundred Years of Successful Fertilizer Practice, *edited by Ludwig Schmitt and Hermann Ertel. Pp.* 258. *J. D. Sauerländer's Verlag, Frankfurt am Main.* 1959. *DM.* 30.

Polythene Film in Horticulture, *by Henry R. Spice. Pp.* 176. *Faber and Faber, London.* 1959. 18*s. net.*

Soviet Soil Science, *Vol.* I, *No.* I, *January* 1958. *A translation of the monthly journal 'Pochvovedeniye' Pp.* 113. *American Institute of Biological Sciences, Washington.* 1959. $7.50.

Tropical Silviculture, *Vol.* I, *Pp. vii*+190. 1958. 10*s. net. Vol.* II, *Pp. xv*+415. 1957. 20*s. net. Vol.* III, *Pp. vii*+101. 1958. 5*s. net. Food and Agriculture Organization of the United Nations, Rome.*

TECHNOLOGY

Organisation of Building Sites, *by R. C. Sansom. Pp. x*+186. *Her Majesty's Stationery Office, London.* 1959. 21*s. net.*

Principles of Modern Building, Vol. I, *Third edition. Pp. x*+302. *Her Majesty's Stationery Office, London.* 1959. 20*s. net.*

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